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GREAT BRITAIN.

ON THE FORMATION OF AN ARTIFICIAL COMPOUND DISPERSIVE POWER IN A COMPOUND LENS, FOR CORRECTING THE FRONT LENS OF A REFRACTING TELESCOPE.

By PETER BARLOW, F.R.S.
Cor. Mem. Inst. France, &c.

THE general principle of correcting the front lens of an object-glass for achromatism is very well understood; it consists only in making the focal lengths of the two lenses proportional to their dispersive powers. When this proportion obtains, and the lenses are placed in contact, the spectrum vanishes, and the object-glass is achromatic; but when it does not, a new spectrum is formed; and by considering the compound lens as a simple one, a new dispersive power is created, and this lens thus formed may be employed to correct another front lens. Also, as the artificial dispersive power thus created may be made either greater or less, in almost any proportion, than that belonging to either of the media by which the compound lens is formed, the correction may be made at almost any distance behind the front lens, and consequently the correcting compound lens may be made proportionally less than the front lens it is intended to correct.

When the compound correcting lens is used in contact with the front lens, it becomes the triple object-glass of Peter

Dollond. When it is made a parallel glass for the me rays, it agrees with the construction proposed by Mr. Roge: and in all its intermediate states, it is the form which I ha proposed for the naturally high dispersive power of the s phuret of carbon.

It is certainly a little remarkable that the original idea of t triple object-glass had never led to this view of the subject; it is obvious that if, instead of comparing the combined foc of the two crown lenses with that of the flint, we compare combined focus of the two back lenses with that of the fro or the two front with that of the back lens, we arrive at a r artificial dispersive ratio of the kind above alluded to, but author of this ingenious arrangement, having had in view o the reduction of the spherical aberration, seems to have l sight of the other advantages he might have derived from same principle.

In my endeavours to supply the place of flint glass by sulphuret of carbon, I was in some measure forced upon t step which Mr. Dollond had failed to take, by finding it nec sary to open the lenses; and this construction immediately called to Mr. Rogers an idea which he had formed some t before of that principle, which he has since published. : neither this gentleman nor myself, any more than the inven of the triple object-glass, seems to have had any idea that had each penetrated by different roads upon the borders c of an extensive field of uncultivated practical optics, whicl duly explored by well directed experimental researches, can fail of producing many valuable results.

While we confine ourselves to crown and flint glass, range is certainly rather limited: if we employ the high fractive glass of Mr. Faraday's manufactory, it is considera increased; but with the sulphuret of carbon it can be so extended as to make the entire correction for colour, in object-glass of any diameter, in the eye-piece of the telesc only. Mr. Coddington has, indeed, remarked, in his highly luable 'Treatise on Optics,' that my correcting lens mus considered as a part of the eye-piece of the telescope; but has reference only to its theory: what I allude to above i be understood also in a practical sense; as it is possible to

duce the entire achromatic correction in a tube one-eighth only of the length of the telescope, and of one eighth of its aperture.

In order to illustrate the above view of the subject, let

$f + \delta f =$ the focal length of the red ray.

$f \quad =$ ditto mean ray.

$f - df =$ ditto violet ray.

In any positive lens, let also

$$f' + \delta' f', \; f', \; f' - d' f',$$

represent the same quantities in another lens, having a different index and dispersive power, where δ, δ' denote the dispersions of the red ray in the two media, and d and d' those of the violet ray. In most cases we consider $\delta = d$, and $\delta' = d'$; it is not so, however, in all cases, and I propose therefore at present to give this generality to the notation.

Confining now our investigation to one side only of the spectrum, as for example the red, and considering the second lens as negative, we have, when the two are in contact,

$$\frac{1}{f+\delta f} - \frac{1}{f' + \delta' f'} = \frac{f'(1+\delta')-f(1+\delta)}{ff'(1+\delta+\delta'+\delta\delta')} = \text{reciprocal}$$

focus of the red ray, and

$$\frac{1}{f} - \frac{1}{f'} = \frac{f'-f}{ff'} = \text{reciprocal focus of}$$

the mean ray.

Consequently

$$\frac{ff'(1+\delta+\delta'+\delta\delta')}{f'(1+\delta')-f(1+\delta)} - \frac{ff'}{f'-f} = \text{length of the}$$

coloured focus; and this therefore, being divided by the mean focus, gives

$$\frac{(1+\delta+\delta'+\delta\delta')(f'-f)}{f'(1+\delta')-f(1+\delta)} - 1 = \text{dispersion of}$$

the compound lens, which after reduction, and denoting this dispersion by Δ, becomes

$$\frac{f'\delta(1+\delta')-f\delta'(1+\delta)}{f'(1+\delta')-f(1+\delta)} = \Delta \ldots\ldots\ldots (1)$$

And here it is obvious,

1. If $\quad f : f' :: \delta + \delta\delta' : \delta' + \delta\delta'$

we get $\Delta = 0$, and it becomes the common double achromatic object-glass; in which $\delta\delta'$ being very small, the proportion is generally given as

$$f : f' :: \delta : \delta'$$

as stated in the first page of this paper.

2. If $$f = f'$$

we have $\Delta = -1$, which is Mr. Rogers' correcting len

3. If $$f : f' :: 1 + \delta : 1 + \delta'$$

the denominator vanishes, and Δ is infinite.

And between these limits lies an immense range of c sive powers, almost entirely unexplored; and out of wl well directed course of experiments could not fail of el many valuable practical results.

If the lenses are both positive, the expression becomes

$$\frac{f' \delta (1 + \delta') + f \delta' (1 + \delta)}{f' (1 + \delta') + f (1 + \delta)} = \Delta. \ldots\ldots$$

In this case Δ can never become zero, but it must sarily fall between the two values of δ and δ', and will quently be less than the greater of the two.

The above deductions, it will be observed, relate only red ray, but by everywhere changing δ into $-d$, and δ' int they apply equally to the violet ray. The expression (I) fore, for the violet ray, with a positive and negative lens,

$$\frac{f d' (1-d) - f' d (1-d)}{f (1-d) - f' (1-d')} = \Delta ; \ldots\ldots$$

and with two positive lenses it is

$$\frac{f d' (1-d) + f' d (1-d)}{f (1-d) + f' (1-d')} = \Delta. \ldots\ldots$$

The former, of course, becomes zero when

$$f : f' :: d - dd' : d' - dd',$$

which, rejecting as before dd', as inconsiderable, gives as red ray

$$f : f' : d : d' :: \delta : \delta'.$$

It follows from these formulæ and equations, that alth in two media we should have

$$\delta' = d', \text{ and } \delta = d,$$

yet, as the combination of the red ray with the mean, require the proportion

$$f : f' :: (1 + \delta') \delta : (1 + \delta) \delta'$$

and the combination of violet ray with the mean, the propc

$$f : f' :: (1 - \delta') \delta : (1 - \delta) \delta'$$

we see at once, in this change of sign from + to −, the c

of that imperfection which is generally designated the secondary spectrum; that is, the same proportion, which combines the red and mean rays, will not combine the violet and mean rays, so that there is necessarily a certain quantity of uncorrected colour when the lenses are in contact, and the dispersions equal on each side the mean ray, and which, with crown or plate and flint glass, is generally rendered worse by the inequality of the values of δ' and d' in the latter; the inequality lying on that side which increases the evil: but I have shown in a paper in the 'Philosophical Transactions' for 1828, that by opening the lenses we have a certain command over the artificial dispersion which offers at a least chance of complete correction.

If, in the above expression, we make $f' = p\,f$, and $\delta' = m\,\delta$, it becomes

$$\frac{p\,(1+m\delta) - m\,(1+\delta)}{p\,(1+m\delta) - (1+\delta)} \times \delta = \Delta.$$

In which Δ denotes, as before, the dispersion of the red ray in the compound lens, δ that of the red ray in the plate or crown lens, and $m\,\delta$ that of the other medium, whether it be flint glass, Faraday's glass, or sulphuret of carbon: m and δ are therefore given quantities, while p is assumable at pleasure within all practicable limits.

At present we have considered the lenses, whose compound dispersion has been ascertained as being in contact; let us now inquire into the circumstances of the compound dispersion due to two lenses placed at a distance from each other.

Let the focus of the first lens be nf, and let the cone of rays from this lens be intercepted by a second lens at the distance f from the focus of the first; then $n\,\delta\,f$ will be the coloured focus of this first lens, that is to say, there will be all the colour due to the focus $n\,f$, but reckoning only from the place of interception the focus being f, we shall have $\frac{n\,\delta\,f}{f} = n\,\delta$ the dispersion of this ray, estimated from the second lens: we have, therefore, only to substitute $n\,\delta$ instead of δ in our former expression, and we shall thus obtain the value of the dispersion of the compound open lens, *viz.*, in this case we shall have

for the red ray with a positive and negative lens,

$$\frac{f' n \delta (1 + \delta') - f \delta' (1 + n \delta)}{f' (1 + \delta') - f (1 + n \delta)} = \Delta; \ldots \ldots . ($$

for the violet ray,

$$\frac{-f' n \delta (1 - \delta') + f \delta' (1 - n \delta)}{-f' (1 - \delta') + f (1 - n \delta)} = \Delta; \ldots \ldots ($$

for the red ray, both lenses being positive, we shall have

$$\frac{f' n \delta (1 + \delta') + f \delta' (1 + n \delta)}{f' (1 + \delta') + f (1 + n \delta)} = \Delta; \ldots \ldots . ($$

for the violet ray

$$\frac{f' n \delta (1 - \delta') + f \delta' (1 - n \delta)}{f' (1 - \delta') - f (1 - n \delta)} = \Delta; \ldots . ($$

where it is assumed that the dispersion is equal on each si the mean ray, as is the case in crown or plate glass, which the medium to which this principle is more particularly ap cable; and here again, as we cannot completely answer b conditions, so as to combine the red and violet with the me ray, we must, as in the case of the common double or tri object-glass, take a mean value of Δ by rejecting $n \delta$ and δ' inconsiderably; this reduces the expressions to

$$\frac{f' n \delta \pm f \delta'}{f' \pm f} = \Delta;$$

the upper signs applying to the case of two positive lenses, a the lower to a positive and negative lens.

Taking this view of the subject, we must, in the combinat of two lenses to correct the front lens of an object-glass, c sider it rather as a combination of the two distant cro lenses, to be corrected by the simple flint lens, at least this be the most convenient form for computation. For examp the focus of the compound lens will be

$$f'' = \frac{f f'}{f' + f}$$

its dispersion $= \Delta$; and calling the dispersion of the flint we have only to compute its focus f''' by the common anal adopted in the usual form of telescope, *viz.*

$$\Delta : \delta'' :: f'' : f'''$$

that is $$f''' = \frac{f f' \delta''}{f' n \delta + f \delta'}.$$

It would extend this article to too great a length to enter into the various cases that arise by giving different values to n and f' in this expression. I shall therefore, simply for illustration, take the particular case proposed by Mr. Rogers, in which it is assumed that $\delta = \delta'$, the first and second lens being both crown, and $f = f'''$, the foci of the second and third lens being equal: this gives

$$f' = \frac{f f' \delta''}{f' n \delta + f \delta};$$

or

$$f' = \frac{\delta'' - \delta}{n \delta} \times f.$$

As an example, suppose the correcting lens to be placed at half the focal length of the front lens; and the focus of the front lens $n f = 84$ inches, and that the correcting lens is composed of crown and English flint glass, of which the relative dispersions are as 2 to 3, that is $\delta = 2$, $\delta'' = 3$ and $n = 2$, consequently, $f = 42$, we should have

$$f' = \frac{3-2}{4} \times 42 = 10{\cdot}25 \text{ inches},$$

that is, the focus of each of the lenses forming the compound correcting lens must be $10\frac{1}{4}$ inches, the whole length of the telescope being 84 inches.

As another example, let the correcting lens be composed of crown and Faraday's glass, of which the relative dispersions are as 10 to 19. Then we should have

$$f' = \frac{19-10}{2 \times 10} f = 18.9 \text{ inches},$$

that is, the focus of each of the correcting lenses must be 18.9 inches.

Lastly, let the correcting lens be crown glass and sulphuret of carbon, the relative dispersions being as 3 to 10. Here we should have

$$f' = \frac{10-3}{2 \times 3} f = 49 \text{ inches}.$$

We see thus the great advantage of a high dispersive power in this form of construction: for, in consequence of the depths of the curves required in the first example, it would be practically impossible to take n greater, or at least very little greater,

than 2; with Faraday's glass, the focus being nearl and the refractive power very great, the curves will be greater radii, and the principle would admit of some able extension; but with sulphuret of carbon it ma been stated in a former part of this article, be pushed to enable us to make the whole correction for colou eye-tube only, that is, in a tube of $\frac{1}{6}$th or $\frac{1}{8}$th the focal l the telescope.

We have seen, for example,—still taking the who 7 feet or 84 inches,—that when

$n = 2$, $f = 42$ and $f' = \frac{7}{6} f = 49$ inches.
If $n = 3$, $f = 28$ and $f' = \frac{7}{9} f = 21\frac{7}{18}$ inche
If $n = 4$, $f = 21$ and $f' = \frac{7}{12} f = 11\frac{3}{4}$ inches
If $n = 5$, $f = 16.8$ and $f' = \frac{7}{15} f = 7\frac{21}{25}$ inches
If $n = 6$, $f = 14$ and $f' = \frac{7}{18} f = 4\frac{2}{3}$ inches.
If $n = 7$, $f = 12$ and $f' = \frac{7}{21} f = 4$ inches.
If $n = 8$, $f = 10\frac{1}{2}$ and $f' = \frac{7}{24} f = 3\frac{1}{16}$ inches,

which latter case even (having regard to the reduce ture) is still as practicable as the flint and crown corre half the focal distance.

With Faraday's glass it has been shown, that with of 84, and with $n = 2$, we have—

$n = 2$, $f = 42$ and $f' = \frac{9}{20} f = 18.9$ inches.
If $n = 3$, $f = 28$ and $f' = \frac{9}{30} f = 8.4$ inches.
If $n = 4$, $f = 21$ and $f' = \frac{9}{40} f = 4.7$ inches.
If $n = 5$, $f = 16.8$ and $f' = \frac{9}{5} f = 3.02$ inche

the case $n = 4$ being perfectly practicable. Thus it a the greatest extension that can be given to this principl flint glass is $n = 2$; with Faraday's glass, $n = 4$, and sulphuret of carbon, $n = 8$: that is, in the first case, a lens of any aperture can be corrected with a flint lens o that aperture; with Faraday's glass, by a lens of one-f the aperture; and with sulphuret of carbon, with a lens of eighth the aperture: and it will be observed that, betwee values of $n = 1$ and $n = 2$, each of these mediums adn unrestricted application—the case $n = 1$ in each being triple object-glass, under that particular arrangement in w the focus of the first lens is equal to the focal length of telescope.

All the above variations may be considered as belo[illegible] one class, distinguished by the condition of f' being [illegible] f'''. Another class belongs to the case of $f''' = 0$, w[illegible] the form I have adopted in my fluid telescope. In [illegible] range is more limited: we cannot here with any ad[illegible] make n greater than unity, while we employ flint-glass. Faraday's glass, we can make n any number between 1 [illegible] and with sulphuret of carbon, any number between 1 and [illegible] the former class the focal power remains throughout the [illegible] as the length of the telescope; but in the latter it n[illegible] increased with the sulphuret of carbon to nearly doub[illegible] length, and with Faraday's glass to $1\frac{1}{8}$ times the length. not, however, necessary to confine ourselves to either of [illegible] classes: we may make $f''' = qf'$, and take q, any nu[illegible] within practicable limits, greater or less than unity; and [illegible] q is less than unity, the focal power of the telescope w[illegible] increased, and with q greater than unity, it will be diminis[illegible]

Hence it appears that the refracting telescope, which [illegible] for about eighty years been limited to one particular form [illegible] admit of an immense variety of untried forms, some of w[illegible] seem to offer important advantages.

In all these cases, for example, the size of the concave [illegible] recting lens, which has hitherto set a limit to the dimens[illegible] of refracting telescopes, may be considerably diminished, [illegible] ing the aperture and light the same; or, which is equiv[illegible] with any given correcting lens, we may employ a much gr[illegible] front lens.

In one class, also, we can, by increasing the focal p[illegible] diminish the length of the telescope; and in another, by [illegible] nishing the focal power, increase the light.

We have, also,—as I have shown in the 'Philosop[illegible] Transactions' for 1828,—at all events a control over the am[illegible] of the secondary spectrum, if not the power of destroyi[illegible] altogether; and, lastly, the error arising from spherical [illegible] ration may be diminished almost without limit.

On the latter subject it may be asked, how (as we de[illegible] or counteract the spherical aberration entirely in the [illegible] telescope) can it be farther diminished? To this I co[illegible] reply, that however completely we destroy it in our ca[illegible] for[illegible]

it is still practically existing in the telescope; that is, we can only destroy the aberration for one index, whereas the perfection of the instrument requires it to be destroyed for three; an error, therefore, must remain, and it is for this reason that, with a given aperture, opticians are compelled to employ a certain length of tube, or rather, not less than a certain length; and, unfortunately, that length increases in a higher ratio than the aperture. The only remedy, therefore, is a reduction of the curvatures; and in some cases included in the above classes, these curvatures may be reduced to one-eighth of those of the common telescope of the same focal power; but to what extent we may in consequence reduce the length of the telescope, can only be satisfactorily determined by experiment.

Unfortunately, such experiments are attended with grea expense; and that expense is rendered greater than is necessary, because many of our observers, however competent they may be to judge of the performance of a telescope, have too little inclination to examine theoretically and judge of principles; and therefore to gain their assent to any new form o construction, the instrument must be perfect, which of cours requires the most perfect glass and the best workmanship, an necessarily creates great expense: whereas, if they were abl or content first to examine principles only, the charge of suc experiments would be much diminished; and when the bes principle had been selected, then, and not till then, I woul incur the expense of perfection.

It is exceedingly difficult, with such a field of inquiry befor one, to say which of all the different cases should be selecte as an individual test. I have, however, suggested a form, an the Royal Society have ordered the instrument to be constructed, which I have every reason to hope will be foun highly advantageous. In this, the amount of spherical error,— or, which I consider equivalent, the amount of curvature,— will be less than one-eighth of what would belong to the common telescope of the same focus. The focal power will excee the focal length, and the sum of all the refractions in th passage of the rays through the lenses, as well as at each individually, will be reduced to a minimum. These appear

me to be important advantages; but there may be others equally important which have escaped me, and over which, had they occurred to my mind, I might probably have been able to exercise some control.

This impossibility of foreseeing all the bearings of an inquiry so completely untried as that which forms the subject of this paper, proves the necessity of an extensive series of experiments to elicit the most useful results; but such a series of experiments, particularly under the disadvantage to which I have alluded above, involves too great an expense to be undertaken at the charge of a private individual. At the same time, I must think that an extensive field of practical optics is here opened, which is highly deserving of cultivation.

ON THOSE BIRDS WHICH EXHIBIT THE TYPICAL PERFECTION OF THE FAMILY OF ANATIDÆ.

By WILLIAM SWAINSON, Esq., F.R.S., L.S., &c., &c.

ALTHOUGH natural history, from having been formerly pursued with an exclusive reference to specific differences, long merited its popular definition of being a science of observation, the attention now bestowed upon the generalization of facts, gives the hope that this science will soon become one of demonstration. It is to be feared, however, that in this eager desire to develope general laws, we sometimes overlook those means by which such results are to be obtained,—that we theorize, as it has been well observed, where we should analyze *, and decide upon the properties of a group, thought to be natural, before we have thoroughly investigated the group itself. The present generation of naturalists, in fact, are as much prone to fall into error from their over-anxiety to generalize, as were those of the Linnæan school, from an exclusive devotion to specific differences, and a total neglect of all the higher objects of the science. This passion for theory, among our own countrymen, dates its origin from the period when naturalists began to study the celebrated 'Horæ Entomolo-

* 'It is the prevalent error of the day, among the naturalists, to attempt to generalize where they ought to analyze.'—Bicheno, Linn. Tr. xv. p. 489.

gicæ;' and when they perceived that the variation of animal structure could only be explained upon the principles there promulgated. But regardless of the warning which the talented author of that work has so often given to his disciples, that nothing is more easy than to form circles upon paper 'provided we do not consider it necessary to prove them,' th fascination of his theory has been such, that some of his fol lowers—forgetful of the discriminating caution of their maste and overlooking those tests which he has himself applied t the only genera he has perfectly analyzed—have ventured t pronounce certain groups to be natural and circular, whicl upon closer investigation, prove eminently artificial. Certai forms are fixed upon as types of structure, before the prin ciples which regulate such types have been either explain or discovered. An arbitrary standard of perfection is th planted, around which are assembled such other species more or less approximate to this fancied point of excellenc as this, however, is founded upon no one fixed principle natural arrangement, every systematist thinks his own ty better than that of his predecessor.

It appears to me, therefore, essentially necessary to stability of any system of zoology professing to be natu that the doctrine of types should be more deeply investigat since, if such forms do actually exist in nature, we are justi in believing they must be regulated by certain general la which, when developed, will be conspicuous in all nat groups of animals. At all events, no arrangement can demonstrative which is in any degree founded upon an assu tion, like that of *types*, as they at present stand. It canno too often repeated, that all true knowledge of zoology exclusively upon analysis—upon a perfect acquaintance only with the organization of a species, but with its ha instincts, and natural history, properly so called: to fi *priori*, upon the type of a genus, or upon the divisions family, before the first has been demonstrated, or the l analyzed, is manifestly erroneous: it is clearly beginni the wrong end; and we do no more than tread in the steps of our predecessors, who first established certain rul their own for making genera, and then referred every

object to these arbitrary divisions. If this error led to so much confusion and to such forced combinations before any pretence was made to discover the natural system, it is fraught with peculiar evil in these days, when a glimpse of the harmonious plan of the GREAT CREATOR is the ultimate object of every zoologist. By fixing *à priori* upon a type, and arranging all the subordinate groups in their several supposed relations to that type, we not only incur the risk of *commencing* in error, but of continuing it through the whole contents of a very large division, and an entire order of animals will thus be inevitably thrown into confusion.

It must indeed be admitted that, with our present imperfect knowledge of the laws of nature, we must, in many instances, be content with theoretical deductions: but we should ever bear in mind, that these theories must be relinquished when opposed to a better knowledge of facts, and the right interpretation of such facts. It has been well observed, that every great discovery has originated from some preconceived theory which has struck open a new path of inquiry, enlarging and expanding the mind at every step. No one will, therefore, deny the importance, much less the legitimate use of theory, as an instrument necessary to develope the greatest truths: the objections are not to its use, but to its abuse. Every naturalist, in fact, who does not confine his attention to the sole study of species must have a theory or a system: it may be borrowed, or it may be new, true or false; still, so soon as he attempts combinations, he becomes essentially a theorist. He sorts and separates his species into parcels or groups, under some definite notions as to the characters they possess. If, after having done this, he proceeds to verify his first impressions by critically examining their correctness, or, in other words, by analyzing his groups, he makes that just and legitimate use of theory which is allowable: but, on the other hand, if he contents himself with investigating the details of one of his groups, and, having discovered certain properties belonging to it, proceeds to apply these results to the remainder, taking it for granted that further analysis is unnecessary, he then invests theory with an importance to which it is not entitled. So

long, indeed, as the world of science is distinctly inform
the arrangement of the one is the result of analysis, ar
of the others is theoretical, no mischief is done: on th
trary, the doubts which would be expressed upon the
particularly by an able and candid writer, will frequentl
to important discoveries. At all events, we should kno
to apportion our confidence; which would certainly be g
upon that group which had been analyzed, than upon
which rested upon mere theory. The inventor of an art
system has no occasion to lay his reasons before the p
but those who propose natural arrangements are impera
bound to candour upon these points. Since no name,
ever great, no style, however persuasive, or no theory,
ever captivating, can be of the least weight when unsupp
by analysis.

The consequences which have resulted from this an
desire to generalize, are no where more conspicuous th
the existing arrangements of the Anatidæ; a family of
to which our attention has been recently directed, as f
ing a considerable portion of the ornithological collections r
by Dr. Richardson, and described in the second volum
the 'Northern Zoology,' now almost ready for publica
This family, whose geographic range is chiefly in the tempe
and arctic latitudes, has long excited the attention of Europ
ornithologists. The species are numerous, and the modif
tions of form so many, that no one group of ornitholog
equal extent, except, perhaps, the Falconidæ, has been
much divided into genera and sub-genera. Upon the valu
these minor divisions, there is, of course, much diversit
sentiment, the inevitable consequence of an opinion alm
universal among naturalists, that nature knows no other d
nite distinctions than those which separate species. The tr
of this, however, has already been questioned*; and it will
my object, upon this occasion, to prove that the views of M
Macleay, in regard to his definition of the term *genus*,
exemplified by him in those of *Scarabæus* and *Phanæus*,)

* Mr. Macleay's Examination, &c. Zoological Journal, vol. iv. p. 406.

substantially correct, and, that a genus, so interpreted, is a definite group *.

The most superficial observer, on looking to the family of the

ANATIDÆ, *or Ducks,*

under which he will include the geese and swans, must be struck by the remarkable shape and structure of the bill, totally different from that of all other birds. This, in fact, is the only group in the aquatic order wherein the bill is very considerably dilated in its breadth, and of a texture unusually soft. In addition to these, a third, and a very important character is discerned: the cutting margins of the bill are provided with numerous transverse lamellar plaits, so much developed in some species, as to project beyond the bill; thus assuming an analogy to the teeth of quadrupeds. This analogy, however, is more imaginary than real, since these appendages are destined for a very different purpose. The feet, although in general short, are adapted to more than one purpose, since they are not only used for swimming and diving, but for walking. The adoption of this structure is in admirable unison with their natural habits, and with the station that ALMIGHTY WISDOM has ordained them to fill in the great empire of Nature. The Gulls feed indiscriminately upon marine animals, whether living or dead: they are the purifiers of the waters, as the Vultures are of the land. The Pelicans and the Penguins derive their support from those large fish which the more feeble Gulls can neither capture nor swallow, while the Terns skim the ocean in search of small fish which rise to the surface. But the inconceivable multitudes of minute animals which swarm, as voyagers assert, in the northern seas, and the equally numerous profusion inhabiting the sides o rivers and fresh waters, would be without any effectual chec upon their increase, but for a family of birds destined mo particularly for that purpose. In the structure, according

* Strange as it may appear, not one of Mr. Macleay's disciples have ad the views of their master on this highly important question. The definition genus, given in the Zoological Journal, vol. iii. p. 97, &c., is diametrically op to that of the *Horæ Entomologicæ*. The one is founded upon abstract reas the other upon demonstration.

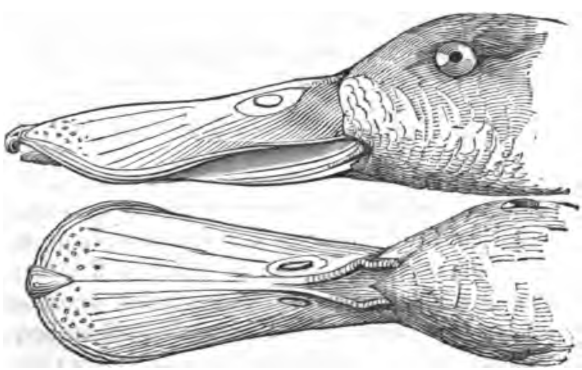
of the Ducks, we see all these qualifications in the perfection. By means of their broad bill, as they fe very small and soft substances, they capture at or considerable numbers. Strength of substance in this is unnecessary: the bill is therefore comparatively w great breadth is obviously essential to the nature of th As these small insects, also, which constitute the chief the Anatidæ, live principally beneath the surface of tl it is clear that the bill should be so formed that the bird have the power of separating its nourishment from tha would be detrimental to the stomach. The use of the thus becomes apparent: the offensive matter is ejec tween their interstices, which, however, are not suff wide to admit the passage of the insect food at the sam The mouthful of stuff brought from the bottom is, as sifted most effectually by this curiously-shaped bill; th is expelled, but the food is retained. It is probable, al the tongue is materially employed in this process; for, that of all other birds, it is remarkably large, thick, and From being so highly developed it must be endowed unusual degree of sensation; and, indeed, a very ex sense of taste must belong to any animal which has t rate its food from extraneous substances, without derivi assistance in the process from its powers of sight: agai deficiency Nature has wisely provided, by heightening creasing the senses of taste and touch. I am acquainted family of birds where this organ is similarly formed, ex that of the *Psittacidæ*. The tongue of the Parrot, as ever knows, is so thick and fleshy as to resemble that of man. however, much shorter and less delicate than that of the and, although endowed, in all probability, with sen somewhat similar, the more immediate purpose of its ture is very different. Those who are familiar with the ners of Parrots, *in their native regions*, well know tha feed not only upon soft fruits, but upon others of the h texture. The seeds, for instance, of the numerous pal Brazil are the most favourite food of the Macaws; a thousands of Parraqueets swarming in tropical America prefer nuts to fruits. This, in fact, is clearly evinced l

great power of the bill. A thick and strong tongue thus becomes necessary, not so much for taste as to assist the parrot in turning round the nut that is to be cracked; it aids the efforts of the bill, supports the fruit in a steady position, and assists in turning it to a more vulnerable part.

It is, therefore, to the formation of the bill, as more particularly connected with the peculiar habits assigned by Providence to this family, that we must look for its typical distinctions; no other of its characters, I apprehend, can be selected, since the rest are common with several other aquatic families. There is nothing peculiar in the diving of Ducks, since the dab-chicks do the same; or in their frequenting both land and water, for so do the Gull family. If, therefore, this line of reasoning be just, it follows that we must esteem that form to be pre-eminently typical of the whole family, which exhibits these peculiarities of the bill in the highest state of development. Every ornithologist will consequently point to the Shoveller Duck as a fit representative of the

GENUS ANAS;

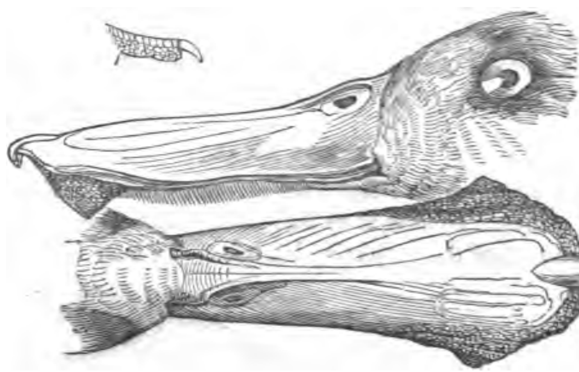
and as a form which differs from all others found in Europe by the uncommon breadth of its bill, and by the delicacy and great development of the projecting laminæ. We have had frequent



occasion to remark*, and to demonstrate the truth of the observation †, that such birds as are typical of a very compre-

* Encyclopædia of Geography, now in the press.
† See 'Northern Zoology,' vol. ii.

hensive division, enjoy an extent of geographic range above all others. The Shoveller Duck of Europe is only found from the northern regions to the table-lanc Mexico *, but is stated to inhabit the Coromandel coast, other parts of India †; while another species, precisel the same form, is recorded as a native of Australia ‡. geographic distribution, then, of the true Shovellers ma termed universal. But among these broad-billed Duck the southern hemisphere, we find a very remarkable n fication of form; the breadth of the bill and the length o laminæ are nearly the same, but the edge of the upper ı dible, instead of being smooth, as in the European spe is furnished with a thin membranaceous skin, which con rably projects, and hangs down somewhat like a wattl



each side. For this form, hitherto uncharacterised, propose the name of

MALACORHYNCHUS,

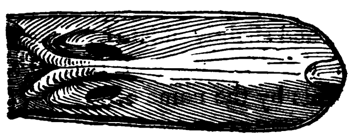
and I shall view it, for reasons hereafter stated, as genus. The bill of the European Shoveller is flexibl in this group it is much more so. One species, descri authors under the name of the soft-billed Shovelle scarcely exhibit this debility more remarkably than which is now before me: it came from the same c

* Specimens communicated to me by John Taylor, Esq., F.R.S., &c., locality, differed not from those brought home by Dr. Richardson.
† Lath. Gen. Hist., x. 312. ‡ New Holland Shoveller, l. x. p.

and seems to be undescribed. Guided by the same views, we next inquire what other ducks present us with the projecting laminæ of the Shovellers; or where we shall look for the gradual diminution of a structure so important to these birds. This diminution we find in the sub-genus,

CHAULIODUS,

of the 'Northern Zoology,' founded upon the well-known Gadwall duck, a bird so repeatedly described, that it is surprising how any part of its structure should have escaped observation. It is, however, certain that this bird makes as near an approach to the Shovellers as any other yet known. The form of the bill, indeed, is no longer spatulate, or perceptibly broader towards the end; but the laminæ of the upper mandible are still very fine, distinct, and more numerous than those of any other form subsequently mentioned, for they project a full tenth of an inch beyond the margin. The tail

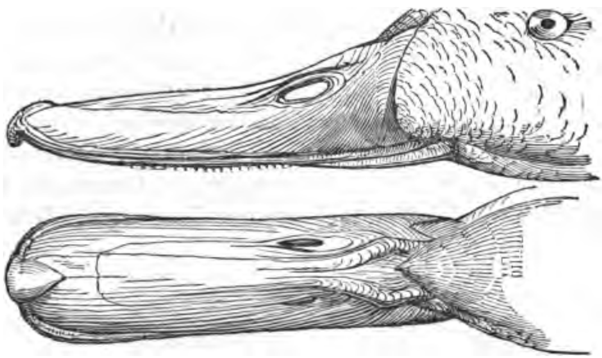


now begins to be lengthened, and, in a new species from Africa (*C. Capensis*), which I have recently received, is so much attenuated, as to evince an evident affinity to the pin-tail duck, forming the sub-genus

DAFILA,

of Dr. Leach. Nature has now so far receded from the typical form, that one of the chief peculiarities of that structure is nearly lost, and another considerably modified. The laminæ of the upper mandible, which, in the *Chauliodus strepera* (Sw.), are so much shorter than those of the true shovellers, and are still more abbreviated in *C. Capensis*, become almost concealed by the margin of the bill in the bird now before us. The most

striking characteristic, therefore, of the genus we are now considering has nearly disappeared, precisely in that form which is furthest removed from the type. But the shape of the bill, although essentially modified, has not undergone a total alteration: its breadth towards the tip is not only as great as at the base, but is even more dilated; so that, in this respect, it resembles the Shovellers more than the Gadwalls, while it differs from both in being higher at its base, considerably more lengthened in proportion, and much more convex throughout.

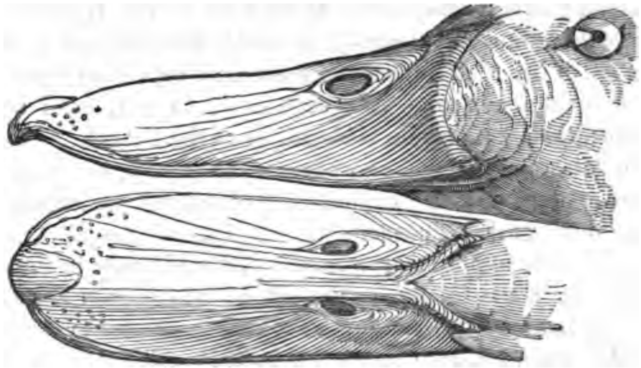


It assumes, in short, a semi-cylindrical form, the end being particularly obtuse and slightly dilated. The precise point of junction between the Pin-tails and that group which was known to the ancients by the name of

BOSCHAS,

has not yet been explained. Under this subgenus we comprehend all those ducks usually denominated Teals, together with the Mallard, long domesticated in our poultry yards. As this is by far the most numerous group, so it exhibits a greater diversity of form among the species. They are all, however, characterised by a bill *longer than the head*, whose breadth is equal throughout; it is sometimes, indeed, a little dilated, but never contracted at its tip, while the laminæ of the upper mandible are entirely concealed by the margin of the bill. The neck and the tail, which in *Dafila* are both considerably lengthened, are much shorter in this group, which is further distin-

guished by the brightness and beauty of plumage observed in nearly all the species.



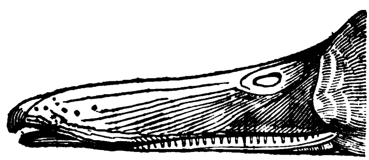
On comparing the bill of the common Teal with that of the Pintail, we see a close affinity between the two forms. But as the tail of the first is so much developed, in comparison to that of the Teal, it becomes essential to discover, if these sub-genera actually followed each other in nature, what species united them more closely. By the uniform liberality of the zoologists attached to the British Museum, and more particularly J. E. Gray, Esq., I am now enabled to do this. The beautiful *Anas* (*Boschas*) *formosa*, Sw., or Baikal Teal of methodists, is precisely a bird which intervenes between these two sub-genera. Essentially a Teal, it differs from all others I have yet seen in the superior length of its tail, the feathers of which are a full inch longer than the under covers*; while the convexity of the bill, from being greater than in the common Teal, establishes its close approximation to *Dafila*.

Proceeding thus by analysis, we find several foreign species which may be either called Teal or Ducks. The *Boschas Javensis*, Sw., is more especially a bird of this description. It is closer allied to the mallard than to any other of the group: this is indicated by the more depressed form of the bill, and the white collar round the neck; the nape also is very conspicuously crested, a peculiarity found in no other group of the

* In *Anas* (*Boschas*) *crecca*, the tail is so short, that the under covers reach almost to the tip of the middle tail feathers.

genus. To this and to the curled tail of the tame
shall presently advert.

Having now reached what appears to be the typ
of *Boschas*, we see that nature, as usual, again depart
The bill of the Mallard is throughout more depressed
of the common Teal. This depression, in fact, fr
greater than that of the Gadwall, or of the Pintail,
assimilates more to the Shoveller. The affinity, how
pears remote, since the laminæ of the Mallard are c
while those of the Shovellers are conspicuously projec
therefore, the affinity was immediate, it could only be
strated by a species having the bill of the common
with the laminæ projecting. Now such a species is
the blue winged Teal of North America, in which thes
project nearly as much as in the Gadwall, while th
mandible exhibits that peculiar sinuosity towards
which is seen in no other ducks besides the Shovellers



affinity required any further support, it is placed beyon
by the fact mentioned in the 'General History of Bir
the plumage of the New Holland Shoveller, excep
white facial crescent, is precisely the same as that of t
winged Teal,—the very bird which thus unites the su
Boschas to that of *Anas*, and completes the circle of th
group.

Zoological circles, however, if founded in nature, re
much better testimony than mere opinion. I have att
to prove, in the forthcoming volume of 'Northern Zo
that the variation in animal structure is regulated by

fixed laws, to the test of which every group thought to be natural must be brought. I shall therefore now proceed to demonstrate the accuracy of the foregoing observations, condensing them into the following tabular form.

GENUS ANAS.

Bill longer than the head, depressed nearly its whole length. The base not enlarged, the tip very obtuse; the laminæ of the upper mandible generally projecting. Hinder toe not dilated, short: claws short, thick. *Fig.* 2, *a.*

	Subgenera.
1. *Typical Group.*	
Bill spatulate, simple; laminæ considerably projecting.	Anas. Lin.
2. *Sub Typical Group.*	
Bill spatulate, furnished with a lobed membrane; laminæ considerably projecting.	Malacorhynchus, Sw.
3. *Aberrant Group.*	
Bill of equal breadth, projecting laminæ short, slender, acute, crowded.	Chauliodus, Sw.
Bill more cylindrical, lengthened; tail long.	Dafila, Leach.
Bill depressed, of equal breadth; laminæ distant, obtuse, and generally concealed; tail short.	Boschas, Antiq.

On proceeding to trace the

ANALOGIES

of this genus, I am aware that it would be more satisfactory to compare it with the other groups of the same order and family. But this could not be done without assuming the correctness of the groups themselves, since the natural divisions of the *Natatores*, and the sub-families of the *Anatidæ*, rest, at present, upon mere opinion;—they have been predicated, but not proved. I must, therefore, content myself with tracing how far the foregoing series partakes of those analogies which belong to the different groups of perching birds; whose circular affinities I have elsewhere attempted to demonstrate. This comparison, indeed, will be much more difficult than the former, since the strength of the analogies between two given groups is always in proportion to the proximity of these groups in the great scheme of nature. It would scarcely be necessary to advert to this obvious truth, did I not apprehend that some writers, who have as yet given but a superficial consideration to the subject, will pronounce these comparisons altogether fanciful. This, however, is not precisely the question. No one

mate and in every country, where man assembles aro the mute companions which live and propagate under the Mallard, almost exclusively, is the only *Duck* whic in this state. How many attempts have been made t ticate others, and how completely, for all practical] have they failed? Thirdly, there is a very elegant and developement of tail in this species, and several othe same group have lengthened crests, at the hinder pa head. Further, this bird is not only more terrestri habits than any we have here noticed, but differs these in frequently constructing its nest at 'some dista the water,' and even in high trees and towers *. In sev cies the under plumage is regularly and beautifully spot in others the tertial feathers are richly ornamented an in an unusual manner. Although the Teals are g considered the smallest race of ducks, yet they are typical examples of this particular group; while the and the other flatter-billed species are, unquestiona largest in the whole circle. I have thus brought t under one view, every peculiarity, whether of exter or of habit, that can possibly be selected as in any wa liar to the Mallard, when viewed in reference to the cular group with which it is here associated; we shall that every one of these facts illustrates, in the most c manner, the analogy of this sub-genus to the rasorial scansorial groups.

The order *Rasores* is remarkable for birds having a bill: it comprehends the peacock, the turkey, the pl and the fowl: all those land birds, in short, whic set apart for domestication by man. In this assemblag we find the most beautiful and singular developmen tail, the most elegant crests, and the most decided parti living on the ground. In this order, likewise, we h most striking examples of a spotted plumage, witness tl

* '*Many* instances are recorded of the common duck depositing her considerable height from the ground: one, mentioned by Mr. Tunstall, sitting upon nine eggs on an oak, twenty-five feet from the ground.—T of the "Rural Sports" also records an instance of a duck taking possess deserted nest of a hawk, in a large oak.'—Montague, Orni. Dict. Sup other instances are mentioned in the popular compilations.

cocks, the guinea-fowl, and the whole family of pheasants; while the plumage of many are lineated in a manner precisely similar to that of the different species of Teal. It is among rasorial birds, that we perceive that extraordinary beauty and elongation of the tertial feathers, which is so conspicuous in this particular group of ducks, and both comprise the largest birds yet discovered in their respective circles. This analogy, in short, which can hardly be rendered more complete, explains, also, the striking and apparently anomalous habit of the wild duck building so often in the hollows of trees, and in similar situations, since this habit more particularly belongs to the whole tribe of *Scansores*, which corresponds to, and represents the order of *Rasores*.

Having now illustrated the whole of these inferior divisions, I proceed to offer the following table as the result.

GENUS ANAS. *ANALOGIES.*

Orders of Birds.	*Tribes of Perchers.*	*Typical characters.*	*Subgenera.*
INSESSORES	CONIROSTRES	Typical in their respective circles	ANAS
RAPTORES	DENTIROSTRES	Bill dilated into a lobe or, tooth	MALACORHYNCHUS
NATATORES	FISSIROSTRES	Pre-eminently aquatic, or flying with great rapidity	CHAULIODUS
GRALLATORES	TENUIROSTES	Bill and neck lengthened, nostrils long	DAFILA
RASORES	SCANSORES	Head crested, margin of the bill entire	BOSCHAS

Each of these three columns, it will be observed, forms a circular group, of which the two first have long been before the public; and although their analogy with the types of form in the genus *Anas*, are of necessity remote, they are nevertheless unquestionable. When we consider, in fact, the great dissimilarity between the groups here compared, we can only feel astonishment that they possessed any one character in common. It would, indeed, as I before stated, have been better had the subgenera of Anas been compared with the groups belonging to its own family, but this would have far exceeded the proper limits of my paper. On a future occasion I may possibly take up another portion of the subject. External structure and natural habits are thus proved to be in perfect unison with each other, and both combine to furnish another proof that the system

of Nature is essentially a system of types and symbols. Se(then that this system can never be developed without the ai those matter-of-fact naturalists,—those true students of nat who throw aside ponderous systems and observe facts, much it is to be regretted that the natural history of birds been so greatly neglected! But for the writings of Le Vai and D'Azara, among foreigners, and those of White, Wil Montague, and Selby, in our own country, the true natu who sought to apply isolated facts to general truths, w receive but little help from all that has been written on the ject *. The most trivial circumstance in the habits and nomy of an animal, may ultimately prove to be just as portant in deciding its place in nature, as any other belor to structure: both are equally essential, both must harmo both confirm, and strengthen each other, and both illus general laws. This has been exemplified in the prec pages, and we can only regret that the natural history of birds (otherwise so well known as species) has not enabl to do this more effectually.

Yet while we give just and due praise to such writer cannot join in the attempt of their admirers, to place upon the highest pinnacle of fame. The study of nature diversified as it is vast, and requires to be pursued by dif modes, and by different capacities. Without the aid c systematist, or of the 'closet' naturalist, the whole book creation would exhibit but a ponderous collection of is facts, interesting indeed in themselves, but crude, unin and trivial to the philosophic inquirer. If natural histor teach nothing more than that one bird built on the groun that another constructed its nest upon a tree, it ma rational recreation, but it can never become a science.

In regard to the tabular disposition of the five sub-gen types of form, given in the preceding pages, it will be ex that I should say a few words, since it is at variance wi mode of exhibiting circular affinities, adopted by that guished writer who first detected this arrangement. C

* Anatomical facts, of course, are equally important with those of h in Mr. Yarrell we can now boast of an ornithologist whose labours in th ment are peculiarly valuable and important.

point, I must refer the reader to the ornithological volume of the 'Northern Zoology,' now about to appear, where he will find our peculiar views explained and illustrated. I have, indeed, chosen to enumerate, in both instances, the subordinate divisions of the aberrant group, but they are always viewed by me as forming a distinct circle of their own, *the primary divisions of every natural group being considered as* THREE, *and not* FIVE. In the present instance, the three sub-genera of *Chauliodus*, *Dafila*, and *Boschas*, possess one common character, in not having the bill conspicuously dilated at its extremity; while their circular succession can hardly be questioned, when we find that the greatest modern reformers* *leave the Gadwall and the Mallard in the same group*:—these writers having overlooked the modification of the laminæ, and passed over the difference in the habits of these birds, as not bearing upon the question.

The theory, that the Mallard is the typical representation of this family, has now, I trust, been thoroughly investigated, and demonstrated to be erroneous; nor can I consider the two circular arrangements† that have been made of the whole family, each apparently perfect, but essentially different, in any other light. They appear to me to be the result of abstract theory, and of a theory completely misapplied. On the other hand, I deem it but justice to the great merits of another ornithologist of our own country, to acknowledge the assistance I have derived from his highly valuable paper, on the tracha of birds‡, and at the same time to declare that if there is a truth in his own inferences, drawn from internal structure, hal in mine, resulting from attention to external form and hab he has himself marked out the true circle of the Anatida far as the British species are concerned, *totally unconscio having done so.* There is, and there cannot be, but one pl creation. In our efforts to develope this plan, we mus Mr. Yarrell justly observes, 'combine ascertained habit ternal characters, and anatomical structure:' and in prop as we can do this, so may we assume that our arrangem NATURAL.

* Dr. Leach, Dr. Fleming, Stevens, Vigors. ‡ Linn. Trans., xv
† Linn. Trans., xiv. p. 499. Zool. Jour. III. p. 404.

ON THE RELATION BETWEEN THE POLYHEDRA SPHEROIDAL THEORIES OF CRYSTALLIZATIOI THE CONNEXION OF THE LATTER WITH THE RIMENTS OF PROFESSOR MITSCHERLICH.

By J. F. DANIELL, F.R.S.,
Professor of Chemistry in King's College, London.

THE Molecular Philosophy, which is the subject following paper, is situated on the very confines of knowledge—on that ill-defined and misty line where th exalted intellect must be conscious that its powers b fail. Towards this debateable region it is, in general, discretion in the practical philosopher not to advance t Within its confines an *ignis fatuus* has too often bee taken for the light of truth, and high energies have wasted which might have brought forth substantial fr firmer ground.

It is for this reason that the professors of the Royal tution have ever acted most wisely in excluding from lessons of chemistry the *Atomic Doctrine*, and have con themselves to the exposition of the theory of Definite Pr tions. Had they followed certain illustrious examples, v have not been wanting, of dogmatizing upon the numbe weight of ultimate atoms, instead of developing the bea relations of chemical equivalents, chemistry, in the plac that captivating simplicity which its aspect now presen our systems, would have exhibited that kind of *pseudo-ma matical* confusion, which, from this and other analo causes, too often obscures the doctrines of other justly c brated schools.

But although this recondite and mental philosophy sh not be mingled up with practical science, there is no rea why it may not be studied apart: on the contrary; a m noble exercise for the faculties cannot be conceived than speculations to which it leads, when bounded by a proper cretion. As science slowly advances we gradually raise point of sight, and we may reasonably expect that our hori will consequently extend; and objects, which before were dimly discerned, will become more distinct, and our judgme

corrected of things in the farthest distance. A more extended view of this description of the ultimate structure of crystalline bodies appears to me to be afforded by the experiments of Professor Mitscherlich upon the expansion of certain bodies by heat; as an introduction to which, a connected retrospect of the two theories of crystallization, although presenting but little new, will, I trust, not be considered as misplaced.

The observation upon which M. Haüy founded his beautiful theory of the structure of crystals is well known. He took a six-sided prism of calcareous spar, and, in attempting to split it, he found that of the six edges of the superior base, three alternate edges only yielded to the blow, and that the division there took place at a certain determinate angle. The three intermediate edges resisted this division; but, in applying the same force to the inferior base of the crystal, the intermediate edges alone yielded. By following up this cleavage in the natural directions thus pointed out, the new-formed planes met together, and he at length obtained an obtuse rhombohedron of definite angles; which was further divisible in the direction of faces into, apparently, an infinite number of similar smaller rhombohedrons.

To this invariable solid he gave the title of the PRIMITIVE FORM of calcareous spar, and he supposed it to be the form of its ultimate molecules; from aggregations of which, externally modified, according to geometric laws, he conceived all *secondary* forms of the same substance to be produced.

This conclusion seemed to derive much force from the observation, that any crystal of calcareous spar, of whatever form, carefully broken, may always be resolved into an infinitude of small rhombohedrons, and that this form persists to the utmost limit to which we can carry mechanical division.

The same observation is applicable to other substances; but the primitive form is, in many cases, peculiar to the substance examined: thus, for instance, a crystal of *sulphuret of lead* is resolvable by mechanical force into a number of small cubes, in the same manner that calcareous spar is resolvable into small rhombohedrons.

M. Haüy calculated the secondary forms of crystals, by decrements of particles taking place on different edges and

angles of the primitive forms. There is no difficulty ceiving a compound cube made up of a large number cubes; but if we place, upon each of the six faces of so formed, layers of cubic particles, decreasing each l of particles parallel to the edges, till a pyramid is cor upon each, terminating in a single particle, the figure converted into a dodecahedron with twelve equal sides. If the decrement take place upon the angles, of the edges, of the original cube, the figure is convei an octohedron.

By decrements of more than one row of particles, intermediate and mixed decrements, it may be shown almost infinite variety of secondary forms may be cons any or all of which may be assumed by the subst which the *primitive* form belongs. Figures of these decrements are now so common in elementary works of that there is no occasion to present them here. So fa servations upon the mechanical properties of such bo go hand in hand with the hypothesis. Parallelopipe six-sided figures, of the nature of the rhombohedron ar might attract one another by their similar sides, wou stable combinations, fill all the spaces which they and would yield to mechanical division only in the dire their joints.

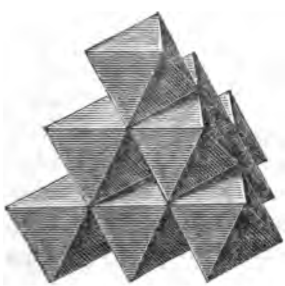
But there is a class of substances, which, affording t series of secondary crystals as sulphuret of lead, yields chanical division in very different directions; and affc mitive forms of a very different character. This class tals is well represented by fluor spar.

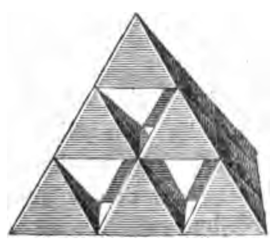
If we apply the edge of a knife with a little dext a cube of *fluor spar,* we shall find that its eig corners may be removed, and that the new-formed pla coincide with those of a regular octohedron. We ma separating slices from any of these faces, all of which split into acute rhombohedrons. Had observation rest no difficulty would have occurred in applying the hyp —the acute rhombohedron would have been the p form—and the cube, the octohedron, and all the modif of this series might readily have been produced by dec

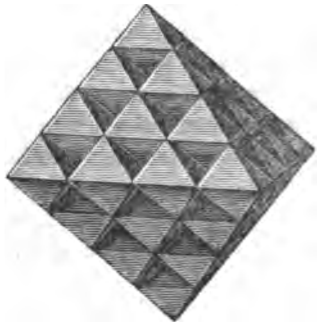
upon its edges and angles. But this rhombohedron, unlike that of calcareous spar, is not only divisible in directions parallel to its six faces, but may be split into two tetrahedrons and one octohedron,—the four solid angles again, if the two tetrahedrons may be split off, and two octohedrons will remain; and the octohedron may be divided into six smaller octohedrons and eight tetrahedrons. Thus the whole mass may be resolved into tetrahedra and octohedra; no one of which can we conceive so small as not to be again divisible in a similar manner.

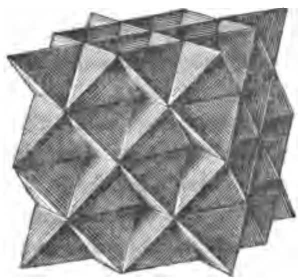
Which, then, of these two solids is entitled to be considered the primitive form of the crystal? Neither of them can fill space without leaving vacuities; and we can scarcely conceive either of them forming an arrangement sufficiently stable to constitute the basis of a permanent crystal.

They may both be symmetrically arranged, so as to afford to the eye the external forms of the series of secondary crystals, which may be geometrically calculated from their various decrements; but they must be conceived to attract one another by their edges only; and the tetrahedral arrangements will be regularly interspersed with octohedral, and the octohedral with tetrahedral cavities. The following figures, which I never yet saw represented in works upon crystallography, exhibit the construction of the tetrahedron, the octohedron, and the cube upon each of these hypotheses.









The tetrahedral arrangement, in fact, represents tl
ties in the octohedral construction; and the octohe
rangement would exactly fill the interstices of the tetr

This appeal to the eye cannot be without its effect ducing a conviction that such arrangements, althoug metrical, must be unstable, and that they are contrary our ideas of the *common* powers of attraction in matter so obviously *impossible,* according to all our experien solids of this kind should attract one another by thei and not by their sides, that we are compelled to a unphilosophical expedient of recurring to an *uncomm* *unknown* power; our ignorance of which will be but cealed, by conferring upon it the name of *polarity,* such indefinite, convenient term.

Another observation here occurs, which I never re to have seen advanced, but which appears to me to be this hypothesis. M. Haüy, in this ambiguous choi primitive form for fluor spar, and a vast variety of oth

talline substances, chose the tetrahedron with octohedral vacuities, rather than the octohedron with tetrahedral spaces, for reasons which he has assigned. Now, if we refer to the figure of *the cube*, constructed upon this principle, it will be observed that, if ever there had been a power which could have thus grouped together these particles, mechanical force would have split the solid in directions parallel to the faces of the cube, and not parallel to the faces of an octohedron; for in such an arrangement each particle is in contact with *one* other particle, while in the second each is engaged by *three;* so that the force of attraction must be greater in the latter than in the former. We thus, in fact, demolish the foundation upon which the whole superstructure is founded.

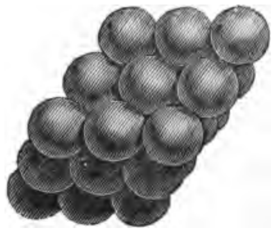
Dr. Wollaston * proposed to obviate the difficulty of the intersticial vacuities in these tetrahedral and octohedral arrangements, in a most ingenious manner. He suggested that the whole difficulty would vanish, by considering the elementary particles to be perfect spheres, and to assume that arrangement which would bring them as near to each other as possible.

The relative position of any number of equal balls in the same plane when gently pressed together, forming equilateral triangles with each other, is familiar to every one; and it is evident that if balls so placed were cemented together, and the stratum thus formed were afterwards broken, the straight lines in which they would be disposed to separate would form angles of 60° with each other.

If a single ball were placed at rest upon such stratum, it would be in contact with three of the lower balls; and the lines joining the centres of four balls, so in contact, or the planes touching their surfaces, would include a regular tetrahedron having all its sides equilateral triangles.

The construction of the acute rhombohedron and octohedron, on the same principle, is as simple as that of the tetrahedron; and the following figures will illustrate the simplicity and stability of the arrangement, and its perfect harmony with the known laws of attraction, both in its construction and the directions in which it would be disposed to yield to mechanical force.

* Phil. Trans. 1813.



Dr. Wollaston next proceeded to inquire what forn
probably occur from the union of other solids most neaı
to the sphere; and he showed that, by supposing the
tary particles to be spheroidical, many solids might
structed which are well known to crystallographers.

By imagining the axis of the *elementary spheroid* t
shortest dimension, a numerous class of well-know
originate. By grouping together oblate spheroids, in t
manner as the spheres in the formation of the acute ı
hedron, the resulting figure will still be a rhombo
but the measure of its angles will be different, and
more or less obtuse according to the degree of oblat
the original spheroid. If the proportion of the axis be
2.87, the rhombohedron will be that of *calcareous spar*

If the degree of oblateness were as 1 to 2, a righ
rhombohedron or cube would result. These solids
obviously be split, by mechanical force, in directions pa
their faces.

If the elementary spheroid on the contrary were *ob*
stead of oblate, it is evident that by mutual attraction th
tres would approach nearest to each other when their ε

parallel, and their shortest diameters in the same plane. The manifest consequence of this structure would be, that a solid so formed would be liable to split into plates at right angles to its axis, and the plates would divide into prisms of three or six sides, with all their angles equal; as occurs in phosphate of lime, beryl, &c.

It is, however, a very singular circumstance, that the construction of the cube with spheres, upon the same principle as the octohedron and the other solids of that series, escaped the ingenious author of this hypothesis—a failure which, had it been essential, instead of accidental, would have rendered it as untenable as that whose defects it was intended to supply.

Dr. Wollaston was perfectly aware that the hypothesis must have appeared defective, if it had not included some view of the manner in which so simple a form might originate; the only mode which occurred to him of supplying this desideratum was, to imagine a mass of matter to consist of spherical particles, all of the same size, but of two different kinds, in equal numbers, represented by black and white balls: these, he suggested, might be arranged four and four above each other, as in the following figure, alternately black and white throughout: the distances of the centres of the black balls, being every way a superficial diagonal of the cube, are equidistant, and their configuration represents a regular tetrahedron; and the same is the relative position of the four white balls. Every black ball is thus equally distant from all surrounding white balls, and all adjacent balls of the same denomination are also equidistant from each other, and the whole might be conceived to be *in equilibrio.*

The experimental part of the investigation had hitherto been confined to the action of *mechanical force;* by the application

of which it was found that crystalline bodies may be cl
certain determinate and constant directions, from wh
planes of least resistance in the solid are easily dete
but this is not the only force by which their structure
dissected. The lower degrees of chemical affinity r
applied, as I have formerly shown, (Journal of the
Institution, vol. i. p. 24,) more delicately to the same p

An irregular mass of alum, which, although to the
exhibits no traces of crystalline arrangement, may ea
shown to possess as regular a structure as the best
crystal of the same substance. Mechanical force will n
us for this purpose, as regular cleavages cannot be d
in it; but if we expose it to the solvent power of
at first the fluid acts upon the salt with so much energ
overcome the cohesion of the solid in every directio
but, as the water becomes saturated, its power diminish
it is nearly balanced by that delicate modification of c
upon which crystalline structure depends. The conse
is, that the solid now yields to the solvent only in the p
least resistance, and the mass will present the form o
hedrons and sections of octohedrons, as it were, ca
stamped upon its surface.

The numerous forms which are thus dissected from th
are arranged in a definite order, with regard to each ot
the different faces of the mass; and the series which
upon one face, and those which correspond with it, ar
intermingled upon dissimilar faces. Thus in one direct
light will be reflected from the faces of octohedrons a
tions of octohedrons all upon the same plane; and by t
the mass upon its axis, the same will be repeated a
quadrant of a circle. By gently inclining the mass,
flection will next arise from right-angled parallelogr
every dimension, which are similarly repeated upon turn
mass upon its axis.

Now, by supposing the process of solution continued
several planes intersected each other, it is clear that
modifications of the octohedron and cube would res
necessarily referable to the same structure of particles
original mass: and it is obvious that each of the almost

external variety of solids which would be produced by the meeting together of the various facets thus presented to the eye must be derivable from one principle of internal arrangement. The *octohedron* therefore and the *cube* could not depend, in this instance, upon such opposite constructions as those represented in Dr. Wollaston's hypothesis. But we are not on this account driven to abandon the spherical atoms, for, however singular it may appear that this eminent philosopher should have overlooked the fact, the cube may easily be shown to be derivable from a structure precisely analogous to the octohedron, the tetrahedron, and the acute rhombohedron.

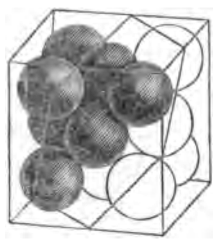
It has been already shown that by placing a sphere upon two opposite faces of an octohedron, we convert the solid into the latter figure: by placing one upon each face, it is as simply converted into a cube as in the following figure. Now,

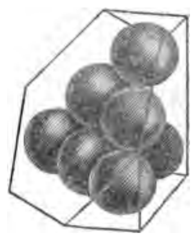
such a cube would obviously be divisible by mechanical force in directions parallel to the faces of the octohedron; because in those directions each particle is held by the attraction of three others only; while in the transverse direction each is engaged by four others; and this is the direction in which fluor spar yields. But now another difficulty occurs—this may be supposed to be a satisfactory account of the construction of the cube with octohedral cleavages, but how shall we explain the octohedron of *sulphuret of lead,* which splits into cubic fragments? This is one of a numerous class of substances exhibiting the same phenomenon of the cubic fracture with the same series of crystals as fluor spar and others of the octohedral class. The theory will be worth little if it should be found applicable only to the latter.

If we suppose ten spheres endued with equal powers of attraction simultaneously exerting their powers upon each other, their forces would be most equally balanced in the *cubic form,* producing the compact and stable arrangemen

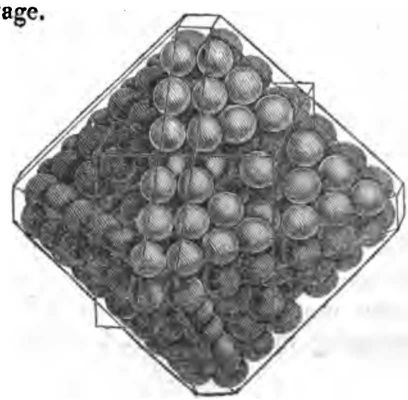
represented in the last figure. If, from some predispo: affinity, the particles of any solution should continue bine in this definite proportion, a number of cubes w formed; which, again attracting one another, would uni ther side by side, according to the general laws which observed. A compound cube would be thus natura structed, and it is evident that mechanical force would such a solid into a number of smaller cubes; for u planes of junction the spheres of one cube are only hel spheres of another cube by the binary attraction of t ticles for each other, while in every other direction ea is in contact with three others at least.

Now, the first simple cube may be resolved into two but irregular tetrahedrons; and if we suppose an oct





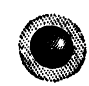
formed by a decrement upon the angles of the cube an to that in M. Haüy's cubic system, these tetrahedrons exactly fill the intersticial spaces; and an octohedral a ment would be formed with precisely the same angles constructions which we have previously considered, but would exhibit the phenomena of the cubic instead of th hedral cleavage.



It would be easy to show how all the regular solids of the octohedral series, and their modifications, might be produced by either of these principles of arrangement; but enough has been premised to introduce the additional argument in favour of the spheroidical hypothesis of crystallization, which may, I think, be unexpectedly derived from the experiments of Professor Mitscherlich.

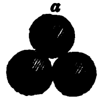
We have hitherto considered the arrangement of our atoms as due solely to their mutual powers of attraction—let us now contemplate it as the result of a balance of the attractive power of the atoms, and of the repulsive power of an elastic atmosphere, with which we may conceive each to be surrounded, and which will represent the repulsive power of heat. The atoms we suppose attractive of each other and of the particles of the hypothetical atmosphere, but the latter highly repulsive of each other.

Upon these postulates, each spherical atom would be surrounded by a stratum of equal depth in all its parts, uniformly distributed over its surface; which, preventing the actual contact of the particles, would nevertheless allow them to arrange themselves according to the laws of the predominant attraction.

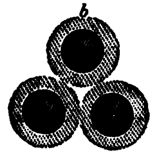


We may suppose the figure *a* to represent a section of the tetrahedral arrangement of spheres in simple contact; and the figure *b* of the same spheres with their atmospheres; an

a



b



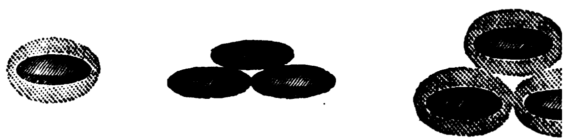
arrangement essentially the same with regard to structure and external figure.

Any addition or diminution of the repulsive *aura* would cause the atoms to recede from or approach towards each other equally; and if we were to heat a solid so constructed, it would expand equally in all directions.

But what would be the case with the structure of oblate spheroids, instead of spheres?

In the first place, as the force of their attraction must, from

the nature of their form, be exerted with greater for direction of their shorter axis than in that of their l taking for granted the two fundamental laws of at first, that all the particles of matter attract one another as their masses, and inversely as the squares of their d secondly, that a body of any shape will attract a p matter anywhere with the same force, and in the san tion, as if all the matter of the body were collect centre of gravity,—it is clear that their repulsive atm will not be distributed in equal layers over their surfa will collect in greater depth above the shorter axis longer; and the atom, with its atmosphere, will assu of the spheric form.



A solid crystal, therefore, thus constructed, must ch measure of its angles with every change of temperatur is precisely what Professor Mitscherlich has ascert happen with crystals of carbonate of lime and other su not crystallizing in the octohedral series.

He found that a rhombohedron of calcareous spar the inclination of its planes to the amount of 8′.5 in th val between 32 and 212. As the temperature augmen obtuse angles diminished; that is, the smaller axis rhombohedron dilated more than its other diagonals, cause an approach to the cubic form.

In substances crystallizing in the octohedral series, h that the expansion was equal in all directions.

The mere inspection of the *rhombohedron*, made to r the primitive form of carbonate of lime, upon the sphe hypothesis, is almost enough to produce a conviction must expand and contract differently in the directions o axes; and the theory might certainly have had the ad of anticipating an observation which tends so powerful support.

A new and interesting field of research has thus been by this third method of disturbing crystalline cohesion

cannot fail of proving fertile of important consequences to the corpuscular philosophy.

There is a great and natural reluctance in the mind to give up an hypothesis ingeniously constructed, like that of M. Haüy, so as not only to present in a general point of view a great number of particular facts, but to enable us to reason from the known to the unknown, and actually to predict facts before trial; and it is from this cause, I think, that the spheroidal view of crystallization has not hitherto received all that consideration which it appears to me to deserve.

It is, however, by no means uncommon in physics to find two theories maintained as to the origin of natural phenomena, both of which cannot of course be the real laws of nature, but each of which will enable us to generalize and group the facts together with accuracy. I need only refer to the two theories of electrical phenomena and the two theories of heat.

When two hypotheses thus run parallel to each other, and each explains a great many facts in common with the other, such unforeseen evidence in favour of one, as I have been endeavouring to explain, is of the utmost consequence. But it is of scarcely less importance to show, if possible, the connexion of one with the other, which enables them to apply to so many circumstances in common.

Now it appears to me that this is not difficult with regard to the two theories of crystallization.

Those who have attentively studied the great work of M. Haüy must be well aware that he could not have been insensible to the difficulties which attended his system, when viewed as an account of the real process of nature; but, whilst in doubt with regard to this ultimate object of inquiry, he most philosophically adopted a temporary substitute for truth, which was capable of leading him by legitimate reasonings to conclusions in exact accordance with observations so numerous as fairly to embrace the whole range of phenomena which the theory was intended to account for.

The fact is, that, so far from insisting on the real existence of his primitive forms and integrant molecules, as many of his followers injudiciously have done, he very early in his reasoning points out the difficulty of the ambiguous choice, and of the vacant interstices; and shows that the whole purpose of

his calculations is answered by imagining the primi in all instances the parallelopipedons (or six-sided which would result from the addition of the vacant s the molecules.

To such imaginary forms he gives the name of *sub molecules*, and he observes that 'the theory would no attaining its principal object, if it were to stop at the p pipedons which the mechanical division of crystals first and the species of anatomy which these parallelop undergo, when we attempt to ascend to the true form integrant molecule, is an ulterior step, without which o tion, rather than theory, would leave something to be The parallelopipedon here represents the unity to which results of the theory may be referred; and it matte whether or not, beyond this unity, there may be fr formed of its subdivisions *.'

With regard to the crystals which derive their origi the octohedron or tetrahedron, he remarks, 'We may c the decrements which give rise to these forms as taking by one or more rows of small rhombohedrons, with an 120° and 60°. Whether the solid parts of these rhombohe be octohedrons, which leave between each other vacuit the form of tetrahedrons, or whether the contrary be the is perfectly indifferent to the theory, which considers *nothing but the rhomboidal spaces, abstracted from the bodies which occupy those spaces*†.'

Now, the spheroidical hypothesis shows how these *ab geometrical spaces* may be filled up more completely, and a greater regard to the known laws of attraction, than b method which suggested itself to the mind of the imm author of the 'Treatise on Mineralogy,' the primary obje whose system is not affected by the change. The calcula founded upon his imaginary *substractive particles* will furnish the key to the different series of secondary forms; we establish, rather than upset, them by showing that may be referred to a system of attractions which, when te by the antagonist powers of heat, chemical affinity, and chanical force, is found consistent with the observed phe mena in every particular.

* Haüy, Traité de Minéralogie, tome i., p. 97. † Ibid., p. 473.

ON THE ILLUMINATION OF THEATRES.

By ALFRED AINGER, Esq.

THE important rank which dramatic representations have ever held among the amusements of all civilized people gives an interest to everything connected with the improvement of even their most subordinate auxiliaries. I have imagined, therefore, that an investigation of the existing methods of illuminating theatres, and a proposition for remedying some of their defects, may not be thought unworthy of a place in a scientific journal.

The first, and this is perhaps the most trifling objection to the present system, is, that it is obviously artificial. Although, ninety-nine times out of a hundred, the scene is supposed to be exhibited by natural light,—that of the sun or moon,—the greater part of the light which reaches the spectator is visibly derived from lamps or candles, which are so conspicuously and variously situated, as of necessity to come more or less within his angle of vision. By this means, not only is the illusion of the scene very much diminished, but its force and vividness, considered as a mere picture, are greatly impaired by the superior intensity of numerous radiating points, which overpower the light merely reflected from the actors and the scenery. A similar injury is occasioned by the unpleasant flickering of the foot-lamps above their screens, and by the excessive light they give to the highly-decorated proscenium, which thus becomes a brilliant and sparkling frame to a comparatively dull picture. The effect is something like what might be produced by hanging variegated lamps round the edges of a painting; and, if avoidable, is scarcely more judicious. The amazing force of the dioramic pictures is mainly owing to the adoption of an opposite course—that of subduing, as much as possible, everything extraneous to the ostensible objects. The central chandeliers, which have been introduced within a few years, produce, to a certain extent, the same sort of mischief, by the powerful light thrown on the ceilings, to which they are of necessity very near, that they may not interfere with the view of the upper spectators. This proximity of the ceiling to the chandelier, and its consequent lightness, are further objectionable, because the first causes the decorations

to be quickly covered with soot, and the second re
visible; so that this part of the theatre becomes
ingly dirty before the remainder is sensibly soiled.
this be guarded against by the use of glass chimney
prove the combustion of the gas, because they would
inaccessible to be cleaned, and they would moreover
ductive of danger to the frequenters of the pit.

In addition to the smoking and flickering of the foot
immediately between the spectator and the scene, they
the sight by the currents of unequally heated air whi
interpose, and which, by irregularly refracting the light,
wavy and disagreeable appearance to whatever is seen t
them. The effect of these lights on the performers is re
evident by the obviously constrained aversion of thei
while the expression of the features is almost destroyed
reversal of the shadows under which the face is usua
best seen. The figure suffers as much as the face fro
inversion; and it becomes peculiarly inappropriate
viewed in conjunction with a scene where the shado
evidently derived from a superior light.

A further objection to all the lights is, that being in si
the audience, they cannot conveniently receive those
tions which it would occasionally be useful to bestow on
and also that their combustion is rendered imperfect l
impurity and agitation of the atmosphere about them.
immense consumption of air adds materially to the *dra*
which too frequently prove prejudicial to the health of de
visiters to the theatre; and the velocity which they lend
general upward current through the roof, together wi
rarefaction and impurity they impart to it, must int
considerably with the transmission of sounds from the sta
that part of the audience which is below or but little
its level.

The mode of illumination which I am about to sugges
if practicable at all, remove the whole of the objections I
enumerated. It will be sensible to the audience only thr
its indirect and intended effect, and it will give approp
shadows to the performers. The light may be obtained u
the best circumstances for securing good combustion;

flames may be supplied with a steady current of pure air; they will therefore burn with little smoke, and neither air nor smoke will ever in any way become obnoxious to the audience. The house may thus be preserved well ventilated and clean, while the lights, by their perfect combustion, will be rendered economical, and still more so by the opportunity of using appropriate reflectors to direct the rays towards the points at which they are required. It is not an exaggeration to say that four-fifths of the light at present employed are wasted in consequence of radiation in worse than useless directions, or of absorption (to use the common term) by imperfectly reflecting surfaces.

I propose to remove the foot-lamps and the central chandelier, together with all the smaller lights round the circumference of the house, and to substitute an illuminated dome, as shown (in the plate) in section, fig. 1, and in plan, fig. 2. This dome would, at its lower part, be formed into circular or octagonal panels, the frames of which might be enriched and gilt, but the panels themselves would be occupied by glass, behind which would be lights of greater or less power, and to each a reflector equal in diameter to the panel, and of greater or less perfection in regard both to form and materials.

In the drawing I have described three tiers of circular panels, each containing thirty-six, or in the whole one hundred and eight. To the quadrant immediately opposite the stage, or from C to C in figure 2, containing twenty-seven panels, I should apply the most powerful gas-lights* I could obtain, and the most perfect parabolic reflectors. Their axes would be in radii of the dome, which would be made such a segment of a sphere, that those radii would point precisely to those parts of the stage where the light was required. It may be thought that light so directed would be too concentrated, or, in the language of artists, *spotty*; but as the reflectors would not average more than eighteen inches in diameter, and the flames would have considerable magnitude, so much of the source of light would be ex-focal, that each parabola would supply a cone of rays sufficiently obtuse to mingle with those

* If it be desirable to employ the light of lime burning in oxy-hydrogen gas, such an arrangement as is here proposed seems peculiarly adapted to the purpose.

of the adjacent reflectors, and blend the whole into o tinguishable *mass*. If, for example, an eighteen-inch be generated, having the distance between the ve focus three inches, and if the diameter of the flam focus be upwards of an inch, the cone of rays issu such a reflector will have at its apex an angle of m twenty degrees. The light thus thrown on the pe would, I think, be greater than could be obtained from seven equal flames in the situation of the foot-lamps lines be drawn from the place of the foot-lights to the position of the actors, it will be evident how extremely sector of the sphere of rays proceeds in an available di while, in the proposed arrangement, nearly every r only so much loss as is occasioned by the imperfectio reflecting surface, would be directed exactly to the points.

The panels in the remaining three quadrants might plied with inferior lights and reflectors; and the glas be ground, to diffuse the light over the house, and to i the appearance of the dome when seen by the audience change from the ground to the unground glass would be only to a small part of the house; and though, as f goes, it would be a defect, it is not, I think, to be competition with its numerous and important adva It is not, indeed, impossible that the light would be sufficiently strong to allow of the glass from C to C slightly ground, by which every objection would be ob If this be not practicable, the defect would be seen only expressly looked for, and would be merely a slight exa tion of the effect which would be produced upon a dome exposed to the sun's rays, which might be nearly cal on the one side, and wholly excluded from the other. variations of light required upon the stage would be ma with much greater beauty and effect, than when a ris setting sun is seen to be accompanied by the rising or s and smoking of a hundred artificial flames.

It will, perhaps, be doubted whether a light, such as I described, would extend over a sufficient portion of the such a doubt will exist only with those who are unaware

small a part of the light on the stage is derived from the foot-lamps. Whenever the performer is behind the line A A on the plan, he is lighted principally from the lamps which are numerous and powerful immediately behind the proscenium, at the points A A; from these also, and from similar lights concealed behind the side-scenes, the scenery obtains nearly all its light, receiving so little assistance from the foot-lamps, that I have the authority of one of the most eminent scene-painters for saying, that if sufficient light can be obtained for the performers, there would be no difficulty in regard to the scenery. I have given all the reasons which occur to me for thinking that it would be sufficient in quantity; and its average direction is indicated by the sword in the hand of the figure drawn on the section.

I have, therefore, no doubt that there would be very little difficulty in realizing the whole of the plan; and that, if realized, the result would be to add much to the comfort, convenience, and splendour of theatres—to give infinitely greater effect to the scene—and to do justice to the features and expression of the performers.

[See Note at the end of the Miscellanea.]

ON THE RED SOLUTIONS OF MANGANESE,

By THOMAS J. PEARSALL,
Chemical Assistant in the Royal Institution.

THE crimson solutions obtained by the action of certain acids upon oxides of manganese, possess some remarkable properties, which have received only partial and unsatisfactory explanations. It has been hitherto supposed that an oxide of manganese existed, which was capable of dissolving in acids, to produce pink or deep red coloured solutions, but the precise state of oxidizement has not been agreed upon. The red oxide, deutoxide, and peroxide have each been selected as the one present*.

In consequence of experiments which I have made to dis-

* Gay Lussac, Annales de Chimie, i. p. 39. Berzelius, Traité de Chimie, iii. p. 298. Thenard, Traité de Chimie, 5th edit., iii. p. 184. Turner's Elements of Chemistry, 2nd edit., pp. 471, 475. Thomson's First Principles of Chemistry.

cover the cause of the colour, and the other propert red solutions of manganese, I am led to believe that th due to the presence of *an oxide*, but to *manganesic a* has not hitherto been suspected *.

These solutions are always strongly acid; they dc stallize or form definite salts; the colours and tints a destroyed by deoxidizing agents, and the concentrated especially if obtained by means of sulphuric acid, a decomposed by mere dilution with water. The solu a peculiar odour, and exert strong bleaching pow effects have been supposed due to the presence bodies, rather than to the state of the manganese the solutions. It was necessary, therefore, at first, tc whether these are constant properties of acid red so manganese, or whether they are owing, as suppos action of chlorine derived from accidental sources.

Black oxide of manganese was repeatedly washe distilled water, which gave no trace of muriatic tested by nitrate of silver; then oil of vitriol, free fror acid, was diluted with its weight of water and poure oxide. In twenty-four hours the fluid had assumed colour, and after several weeks a very deep crimso was obtained, which, when diluted, produced no turbi nitrate of silver. A dilute solution of sulphate of i powerfully bleached by this red fluid, and a stronge of indigo had its colour instantly destroyed, leavin amber tint. These bleached portions of fluid gav least indication of chlorine when tested; but a solutio bleached by *chlorine* gave a precipitate with nitrate o

Part of this red solution was introduced into a retor of which was dipped into a solution of sulphate of ir the retort heated by a spirit-lamp; the indigo was not neither was there any trace of the odour of chlorine ration was continued until part of the contents of the distilled over, the temperature having been raised to a

The retort was then heated with the neck imme solution of nitrate of silver, in which no cloudiness

* Brande's Manual of Chemistry, 3d edit., ii. p. 6.

and the fluid in the retort still strongly bleached indigo; but when a small portion of solution of chlorine was added to the same crimson fluid, or to colourless protosulphate of manganese, and then heated in the same manner, chlorine was driven over, and rapidly bleached the blue liquor. If chlorine, therefore, had been present in the previous experiments, it would have been rendered evident by this arrangement.

In other experiments the crimson sulphuric acid solution was decomposed by being much diluted with water, the oxide separated by filtration, and the clear liquor distilled; half the fluid, however, passed over into the indigo without effecting any bleaching change, nor did the liquor in the retort bleach: if chlorine had been present in the solution, it would have remained after dilution.

Then pure hydrated protoxide of manganese, which had become brown by exposure to the air, was mixed with sulphuric acid, and a red fluid was obtained which possessed bleaching properties, but which, when diluted with water or heated with alcohol, lost all colour and all bleaching power: hence there is no evidence that chlorine is the bleaching agent; on the contrary, the bleaching power of the sulphuric solutions accompanies the coloured state of manganese, and it appears that these two properties are present or absent together.

As the red sulphuric solution appears to have been the only one referred to with regard to this power, I proceeded to examine whether other red solutions of manganese possessed similar properties.

In the process of triturating together binoxalate of potassa and peroxide of manganese, pointed out by Van Mons*, a crimson fluid is produced of great depth of colour; it is acid, and becomes colourless after some time, depositing crystals. I found that while red it bleached indigo very strongly, the action being accelerated by the addition of sulphuric acid.

This crimson solution lost its colour when heated in a retort; carbonic acid gas was evolved, which bubbled through the solution of indigo, without altering it, and the fluid in the retort, now clear and colourless, had lost its bleaching power.

* Quarterly Journal of Science, ix. 409.

When nitrate of silver was added to this crimso an abundant white precipitate of oxalate of silvei duced, which was readily soluble in diluted nitric thus easily distinguished from the chloride of silver, it might be mistaken from its similar appearance; ı cially as the oxalate, like the chloride of silver, is pure ammonia.

Hence it appears that these red solutions possess ing powers in consequence of the peculiar state of n and independent of chlorine *.

When pink or crimson solutions of manganese, sι contain red, deut, or peroxide, were compared with known to contain manganesic acid, their similarit properties was so striking, that I was induced to sι manganesic acid alone ought to be regarded as the c peculiar effects.

1. The varieties of scarlet, crimson, or purp belonging to manganesic acid, in different circumst be imitated by those from sulphuric acid and oxide ganese. 2. The red solutions of oxides are al acid; manganesic acid is soluble and compatible w 3. The red solutions by oxides and sulphuric ac manganesic and sulphuric acids, mixed, also bl strongly. 4. The crimson solution by the actio oxalates upon oxides of manganese bleaches indi ganesate of potassa with binoxalates also does 5. Both kinds of solutions are alike rendered col the same deoxidizing agents. 6. Both kinds of are subject to decomposition by mere dilution w 7. The sulphuric solution evolves a peculiar odou nesic acid in vapour has a similar odour. 8. The certain metallic salts to the supposed solutions of o also to solutions holding manganesic acid, give simil ances; and indeed the similarity of the two sets of

* Mac Mullen on the Native Black Oxide of Manganese—Quarte Science, xxii. 233, xxiv. 261. Phillip's Observations on Mac M of Phil., and Phil. Mag. N. S., i., 313. F. W. Johnstone on the Chlorine from the artificial Oxides of Manganese—Quarterly Journ xxv. p. 154. Kane on Existence of Chlorine in Peroxide of Man p. 286.

solutions is so great as to offer the highest probability that their powers depend upon a common cause.

It will now be my endeavour to place clearly the experimental reasoning which supports this new view of these solutions; and, as the bleaching properties of the ordinary red solutions has been first brought into consideration, I will show that the same effects may be produced by manganesic acid under the same circumstances.

A solution of manganesic acid in sulphuric acid was moderately heated in a retort; the portion distilled over did not bleach, but the fluid, still red, bleached indigo instantly. If much sulphuric acid be present, the mixture may in this experiment be heated for some time, and sustain an elevated temperature before the separation of oxide.

Sulphuric acid was added to chamelion mineral, and produced a deep crimson solution of the red manganesate of potassa, that instantly bleached a strong solution of indigo. This red fluid heated in a retort evolved volatile matter, which destroyed the blue colour; but there was no trace of chlorine in the bleached portions of indigo when tested by nitrate of silver.

These experiments with the sulphuric acid fluids containing manganesic acid, prove that this state of manganese produces the same results as the crimson solution. That the sulphuric acid was not essential to these effects was thus shown: by dissolving mineral chamelion in water it gave a deep green solution, which, when boiled, became deep red, and then bleached sulphate of indigo; the resulting fluid was unchanged by nitrate of silver: an aqueous solution of manganesic acid produced the same effect.

The crimson solution obtained by the alkaline binoxalate and oxides of manganese is the only other particularly pointed out as supposed to contain the deutoxide of manganese; although acid, it is not so strongly acid as the sulphuric fluid: on adding oxalic acid or binoxalate of potassa to solution of green chamelion, the rich colours of manganesic acid appeared. This deep crimson solution is almost identical in colour with the solution in the former oxalic experiment, and, like it, also bleaches indigo.

Solutions of green chamelion and binoxalate of pot
heated in a retort; the fluid soon became colourless,
out changing the dilute solution of sulphate of in
which the neck of the retort was introduced: the
fluid was incapable of affecting indigo.

Manganesic acid, mixed with oxalic acid, very
bleached indigo: neither oxalic acid nor the binoxal
any bleaching power over indigo, but manganesic
some of its combinations possess this property.

The colourless oxalate of potassa and mangane
remained in the retort after distillation, was rendere
sulphuric and oxalic acids; and then, upon the a
manganesic acid, a clear red solution was formed, v
strong bleaching powers; but it lost this property up
ing colourless, which it did in a short time. A ver
trated solution of red manganesate of potassa was
another portion of the same colourless triple oxalate
colour was more permanent, and the bleaching
energetic, than in the preceding experiments. Th
afford experimental evidence that manganesic acid
ble of reproducing the characteristic properties of
solution supposed to contain a deutoxide.

I shall now submit that the known properties of m
acid and protoxide of manganese are capable of expl
action of acids upon the various oxides of this metal
istence of either the red, deut, or peroxide in these flui
to me to be an assumption:—of course, the proto
always be considered as present, according to the us
ing upon the relations of oxides to acids. The admi
of sulphuric acid upon peroxide is to form the prot
thus leaves the formation of the red solution by eith
or the deutoxide quite unaccounted for; therefo
be advantageous to examine the changes which m
posed, in order to produce either of these oxides
it may be assumed, that the acid reduces the whole
oxide acted upon to the state of red oxide, or deuto
forms a red solution; or, 2dly, the acid reduces part
oxide to the state of protoxide, and another part t
of red or deutoxide, and both are in solution tog

3dly, the formation of these oxides may be accounted for by supposing the peroxide reduced to protoxide, portions of which become again oxidized by oxygen evolved from other portions of peroxide. Since the affinities of these oxides are admitted to be so much inferior to the protoxide, which, with acids, produces definite and permanent compounds, it is not to be expected, therefore, that either the red or the deutoxide should alone be produced; and in the second, which is the simplest case of the action of sulphuric acid, where, by the loss of one proportional of oxygen, a protoxide is produced to form the constant base of the sulphate, there is no reason to suppose that another oxide should be formed at the same time, and held in solution by the same acid, but with which it is admitted that it cannot form salts; on the third view, the effect of additional oxygen, combining with the protoxide, would be to produce another oxide, acknowledged to have much less affinity for the acid than the protoxide, which, in fact, is to suppose that a weaker could subvert a stronger affinity, and that the feeble indefinite combination resulting (so feeble that even water can destroy it) could hold the place of a strong, definite, and neutral compound with the same acid. Even admitting that protoxide of manganese could combine with oxygen to form a coloured state, this notion would certainly be in favour of manganesic acid. So I suppose that protoxide is present in the red solutions; and it will presently appear that manganesic acid and protoxide may be in solution together.

On the supposition heretofore entertained, that an oxide is the cause of colour in the red fluids, it has not been stated whether this oxide be alone or with protoxide; but it has been admitted that when a red solution by decomposition precipitates a dark oxide, that much protoxide remains behind in solution *. Then this precipitate evidently does not contain the whole, or the same proportion of metal and oxygen which existed in the red solution. It has also been considered that the colour was due to the oxide thrown down; and as no mode has ever been pointed out for separating protoxide only, it follows, that when the manganese is precipitated from a crimson

* '3 grains of peroxide were precipitated, and after the action of water potash threw down 27 grains of oxide.'—Phillips, Phil. Mag., N. S., v. 216.

solution by an alkali, it will consist of the mixed
previous solution; and hence, because a brown or
may be thus obtained, it cannot be admitted as a]
this is the same state which existed in the solution
the contrary, the same precipitated oxide when dis
acids being resolved into the same states as before, t]
crimson fluid obtained from a red or brown oxide is
no proof that the same oxygenated state of manganes
into solution which was acted upon by the acid; so
seems no satisfactory evidence of either the red, de
peroxide in the crimson solutions of manganese. '
stant presence of either the red or deut oxides seem
cilable with the fact, that every degree of oxidizeme
than the protoxide will afford red solutions; this,
agrees with the production of manganesic acid.

It has often been remarked as a singular circumst
so small a quantity of the red or deutoxide should b
of causing deep tints*; but these observations wi
with the fact, that a minute portion of manganesic
produce intense colour. The experimental formatio
theoretical composition of manganesic acid afford a
in favour of its presence in the ordinary red soluti
Forchammer † first separated this acid from bases;
cess which he employed consisted in precipitating a s
green chamelion by nitrate of lead: he describes
brown precipitate as a mixture of peroxide of
deutoxide of manganese, which, by digestion with
acid, formed sulphate of protoxide of lead, while th
given off united to the deutoxide of manganese to fo
ganesic acid; here there is a mixture of oxides, one
is resolved into protoxide. Now, if we substitute pe
manganese in the place of peroxide of lead, then, by
the action of sulphuric acid is, as before, to resolve
into protoxide, while the oxygen given off unite
deutoxide to form manganesic acid; and upon this
changes may be expressed as follow: each proportion
oxide by the action of sulphuric acid loses one pro

* Dr. Turner, Phil. Mag., iv. 31; and Phillips, Phil. Mag., N. S.

† Annals of Philosophy, xvi. p. 133.

of oxygen to become protoxide, whilst two proportionals of oxygen so evolved unite with one proportional of peroxide and constitute manganesic acid, which instantly assumes an independent existence in the acid solution.

I have mentioned that the tints are not constantly the same when oil of vitriol acts upon peroxide of manganese in the cold, the colour, at first pink, becomes ultimately deep crimson; a rich scarlet fluid is formed when sulphuric acid acts upon hydrated brown oxides; and I have also observed, when excess of oxide has been employed, that the subsequent additions of acid were paler and pinkish tinted. If strong oil of vitriol be added to the deep crimson solution, the colour changes to pink or to violet, bordering upon purple: on concentrating the same deep red solution by heat, it changes to pink tint. These variations are enumerated to show that no arguments can be raised against manganesic acid on the point of colour: for these colours are identical with those exhibited by manganesic acid and manganesates under different circumstances.

Assuming that manganesic acid gave colour to the red sulphuric acid, then the addition of manganesic acid should increase the colour and other properties; and it was found that manganesic acid added to pink sulphuric acid decanted from an oxide immediately heightened the brilliant pink tint, giving deeper colour without any other change. After the fluid had been kept three months in a stopped bottle, the pink colour still remained stronger than that of the acid originally employed. The red manganesate of potassa, with sulphuric acid, was also mixed with another portion of the same solution; the colour was increased to crimson; and although oxide of manganese was deposited upon the glass after some days, the tint still remaining was stronger than that of the acid in contact with oxides. A portion of the red sulphuric solution was decomposed by adding six times its bulk of water; dark brown oxide was separated; and the clear fluid evaporated to its former bulk; then red chamelion of potassa restored the red colour, which appeared exactly like that of the original fluid when they were compared. Two mixtures were prepared—one of sulphuric and manganesic acids, the other of sulphuric

and colourless protosulphate of manganese; both v similar temperature and density; they produced a cleaı red solution when put together, which, by dilutioı water, decomposed, and appeared of an amber tint separation of oxide. Protosulphate of manganese n added to red manganesate of potassa and sulphuri without any immediate precipitation. The triple sulpl ammonia and manganese, when acid, was rendered pi manganesic acid. The existence of manganesic acid in containing protoxide, at once explains the origin of th tints observed in the salts of manganese.

These experiments relative to the addition of mang acid to the ordinary red solutions, show that the mixture bits the same colour as a solution of the same depth o either of sulphuric acid and red manganesates, or of the nary red sulphate; but the properties so communicated i case clearly depend upon the known state of manganese therefore the evidence is much strengthened in favour c opinion that the whole of the phenomena of all red solı of manganese are due to manganesic acid.

Having thus found that manganesic acid could do all the red solutions performed, I endeavoured to obtain the itself from solutions formed in the usual mode, by aci oxide; and as manganesic acid is capable of existing state of vapour, I hoped, if it were present, that a small tion might be volatilized by distillation. I had previ found that the bleaching alkaline chlorides could hold ma nesic acid in solution; very many distillatory experiments therefore made with the very deeply coloured red sulphate, in some of them it appeared as though manganesic acid driven over into the receiver, for a solution of indigo bleached; and even the acid itself, by its pink tints and c properties, was evident in the solution of chloride, which ceived the extremity of the retort. These results bec doubtful, when it was afterwards found that certain proport of acid protosulphate of manganese when mixed with a s tion of bleaching chloride of soda *formed manganesic a* The experiments were therefore repeated with a retort ha a long neck, which was bent several times in order to col

any acid fluid during the distillation; and according as greater precautions were taken, slighter indications of manganesic acid were obtained; but as the hot fluid which collected in the angles would obviously tend to absorb and interfere with any small portion of volatile matter from the body of the retort, and as the length of transit was also increased, under these circumstances no conclusion can fairly be drawn either way. While the above experiment throws doubt upon the results of vapourizing the crimson fluid, yet it establishes the fact, that manganesic acid is produced when oxides of manganese are in solution with substances evolving oxygen, and therefore supports the particular view I have taken*.

Experimental comparisons were then made between the red solutions and those containing manganesic acid, with regard to other properties; but the presence of protoxide of manganese in the red solutions, and the great excess of acid, are circumstances which interfere with and modify the results obtained by the action of some tests, for as yet no process has been devised for separating protoxide or the proto-salts.

Solution of green chamelion of potassa and oxalic acid formed a deep red solution, as also did red manganesate of potassa and binoxalate of potassa. Green manganesate of baryta and binoxalate of potassa produced a rich red solution. These were compared with the crimson fluid from a mixture of binoxalate of potassa and peroxide of manganese. All these solutions became colourless in some hours' time, and in concentrated solutions deposited white crystals. They are all rendered colourless by heat. Ferro-prussiate of potassa gave in all a peculiar yellow-green precipitate; hydriodate of potassa a reddish-amber tint in all. Red ferro-prussiate of potassa occasioned in the whole a similar red-brown turbidness; by transmitted light the edges appeared greenish. With tincture of galls all these solutions became colourless, and deposited light-brown oxide. Proto-muriate of tin rendered them colourless, and precipitated minute white crystals. Sulphuretted hydrogen destroyed colour in all, and rendered them turbid. Caustic

* Dr. John described the fact of the volatilization of manganese by distillation some years before manganesic acid was known; but he supposed it to be a new or different body, and remarks that the experiments must be made upon some pounds of the ore at once.—Annals of Philosophy, ii. 270.

potassa, soda, or ammonia, threw down brown o: them all.

Hydriodate of potassa produces no change in solut proto-salts, but it destroys the colour of manganesate ducing an amber tint of free iodine. The oxalic red i similarly affected, and is identical in colour with the re(ganesates, if oxalic acid be added to them.

Both manganesic acid and the manganesates mixe oxalic acid give a bright yellow-green precipitate with pr of potassa; a similar precipitate may also be obtained ordinary oxalic crimson solutions.

By alcohol, the solutions of manganesates and binox afford red crystals. The crimson fluid supposed to hold oxide also throws down red crystals, which appear to precisely the same properties.

Thus, three solutions containing manganesic acid, ε red solution supposed to contain deutoxide, have similar perties.

I have not succeeded in forming red solutions by a with muriatic acid upon oxides of manganese. Diluted n atic acid and green manganesate of baryta gave a cri solution, which, when added to concentrated and ne muriate of manganese, threw down brown oxide; but a c son transparent fluid resulted when the muriate had exce: acid. Muriatic acid changes green chamelion red, and mu of manganese may then be mixed with it*. Nitric acid oxides do not form a red solution, but strong manganesic a communicates pinkness to the solutions of proto-nitrate manganese. All these solutions very soon become scarlet : turbid: however, these experiments show that mangan acid, protoxide of manganese, potassa, or baryta, may e:

* H. Rose (Treatise on Chem. Analysis, pp. 91, 92) describes the propertie a *dark brown* solution of deutoxide of manganese in muriatic acid; and says t 'by boiling, the perchloride is rapidly reduced to protochloride.' It is also st that 'the peroxide of manganese dissolves in the cold acid, and also produce dark brown solution of deutoxide of manganese.' It may be sufficient for m observe, that, as these are not *red solutions*, they need not be considered as oppo to the view which I am supporting; and even if they should hereafter be pro to contain dissolved deutoxide, then it is obvious that this oxide will give a *d brown*, and *not a pink or crimson* solution. Finally, the perchloride of mangane when decomposed, forms muriatic and *manganesic acids*, according to M. Dum who discovered this compound.—Edin. Journ. Science, xv., p. 179.

together to a certain extent by excess of acid, which co
accompanies all the ordinary red solutions of manganes
Acid solutions of sulphates of potash, soda, magnesi
zinc, appear of a clear red colour after mixture with
sulphuric acid, or with manganesate of potassa. The
of potassa and soda are also reddened by both solution
the red sulphate, or the crimson oxalic fluid from oxide
have their colour increased by the addition of mangane
potassa. Thus both sets of solutions are similarly com
with other fluids.

When a solution of chloride of lime (bleaching pow
added to proto-salts of manganese, the protoxide, by the
of chlorine, receives additional oxygen, and is convert
peroxide. On using the muriate, I observed that, afte
days, the supernatant fluid became bright pink; and, ex
that protoxide of manganese would be absent from this s
I examined it more particularly. The colour, increa
time, became pink and violet, like a solution of pure m
nesic acid, which I found was present, for potassa preci
lime, and then changed this red fluid to blue and gree
melion. I have obtained red fluids in this way several

The solutions of the bleaching chlorides of potassa an
have frequently pink tints, supposed to be due to the int
tion of manganese in some unknown state into the
while chlorine gas was passing through the apparatus.
tically it was found that the fluids could only be ob
colourless by slowly disengaging the gas; for wheneve
chlorine was rapidly evolved, if the solution was caustic p
or soda, it always became pink or red.

I procured two specimens—the one was colourless, an
other pink; and I added an aqueous solution of manga
acid to the colourless fluid, which instantly assumed a si
but much deeper tint than the coloured fluid: then th
loured fluid had its colour greatly increased by manga
acid; neither fluid exhibited any other change by such
tion. I found that manganesic acid and manganesates

* Mr. Phillips, alluding to the same fact, says, that 'the muriate of man
should be as nearly saturated as possible, for the chlorine evolved by ex
muriatic acid occasions the acidification of a portion of the manganese.'—
of Phil., N. S., v., p. 216.

compatible with all the bleaching chlorides of causti carbonated alkalies: in some experiments the tints unchanged after four months.

On the supposition that an oxide is the cause of the known tints of manganese, it seems difficult or impossi account for the introduction of any of the oxides into fluids; for neither the prot, red, deut, or peroxide, or ε their compounds, are volatile; and even if one of the admitted to be present, it could not be retained in these tions, because they are decidedly alkaline. Now, on the which I advance, manganesic acid can exist in solutio carbonates and bi-carbonates, and in these bleaching solu Manganesic acid is volatile, as are the perchloride anc fluoride of manganese, which, on decomposing, form ma nesic acid*.

These facts I consider will satisfactorily explain the hit almost anomalous appearance of manganese in certain of experimental research, and in various processes in arts †.

From all that has been advanced, it would appear bleaching properties, which have sometimes been attribute chlorine, in certain cases, belong to all red solutions; that these solutions are similar to such as contain mangan acid—for *both* solutions are alike in colour and in bleac power; *both* become colourless by the same agents; *l* lose the bleaching power by losing the coloured state; *l* afford similar indications by reagents; *both*, while red, af a red salt in crystals, which appears to possess the same perties; *both*, when they lose colour, afford a crystalli

* Quarterly Journal of Science, xxv., p. 486.

† Manganese was observed in a solution of carbonated alkali, into whic had been carried by chlorine, although the gas had been washed with w and had also passed through an alkaline solution, before the oxide was depos in the second bottle of Woulfe's apparatus.—Quarterly Journal of Science, xxv., p. 86.

Chloride of lime is frequently obtained which dissolves with a pink colour to manganesic acid, although the chlorine gas had been transmitted thro water.

Mixtures of chlorides of lime and potassa, prepared for some manufactu purposes, constantly afford a deep red solution, and possess *extraordinary* ble ing powers.

Manganesic acid also colours the solutions in the formation of the salt cal chlorate of potassa.

colourless proto-salt; *both* are compatible with certain other solutions of other substances. From these close and numerous points of comparison, I conclude that the effects of *all red solutions of manganese depend upon manganesic acid.*

The production of manganesic acid in the cases investigated is agreeable to theory; and if its presence be considered as established, some important consequences follow. The soluble states of manganese are thus resolved into colourless protoxide and manganesic acid. When oxygen is liberated from peroxide by sulphuric acid, manganesic acid may be alternately produced and destroyed. The composition of any oxide which has been or may be obtained from a red solution will obviously depend upon the quantities of protoxide and manganesic acid; and this explains the variable proportions which have been obtained with such precipitates.

The view I have taken may have many interesting bearings. Thus the red oxide is composed of 28 metal and 10.66 oxygen, and the deutoxide of 28 metal and 12 oxygen; and these are assumed to form with acids very feeble indefinite compounds, existing only in the cases referred to, and not volatile. On the contrary, manganesic acid, the cause now assigned of the redness of the solutions, is constituted of 28 metal and 32 oxygen: it is capable of existing in the solid or anhydrous form, or in the state of vapour, and so be transferred from one situation to another. It is soluble in water, in certain alkaline fluids, in some acids, and in solutions of many saline compounds. It is capable of combining with alkalies and earths; when decomposing, it can impart oxygen to other bodies, and thus produce or modify effects which may have been referred to other causes, when its presence has not been suspected.

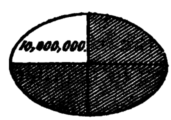
I have given the evidence as much as possible dependent upon the *qualities* of the solutions, and purposely so, in order that the view now brought forward may not be interfered with, should the *proportions* of any of the compounds be subject to correction. Having drawn my conclusions from experiments, I have adverted to the opposing statements of authorities only, to show the opinions entertained upon this subject of acknowledged difficulty.

ON THE COMPARISON OF BRITISH, FRENCH, DUTCH WEIGHTS.

By Dr. G. MOLL,
Professor in the University of Utrecht.

(Communicated by the Author.)

IT is well known, that after the conclusion of the la
the Commission for the establishment of the metr
tem of weights and measures in France, in 1799, dupl
the mètre and the kilogramme were presented by the
Institute to each of the foreign members of the said (
sion, and also to each of the Governments then
France, who had sent commissioners, on this occa
Paris. These duplicates were authentic copies botl
mètre and kilogramme, made and adjusted, it is sai
the immediate inspection of the Commission of Weig
Measures, and carefully and minutely compared
original standards. After this, they were marked wit
ticular stamp of the commission, being an ellipse divi
four quadrants, three of which are shaded, and in the
not shaded, the number 10,000,000 is engraved; t
form of this stamp is nearly the following.



The late Professor Van Swinden was one of the C
sioners sent by this country to the general meeting a
and in consequence he became possessed of a set
authentic copies of the mètre and kilogramme, on w
always set the highest value, and which were constar
served with the greatest care. After Mr. Van Swind
mise they came into my possession.

Being furnished with one of the well-known bala
Mr. Robinson of Devonshire Street, and with very
imperial British troy weights, made by the same artis
anxious to compare Mr. Van Swinden's authentic cop
kilogramme with the British troy weight.

The beam of the balance which I employed is about eighteen inches in length; the knife-edge of the axis is about two inches and a half long; the knife-edges on which the scale pans are suspended are about one inch. When loaded with a kilogramme in each scale, $\frac{5}{1000}$ of a grain are sufficient to show a derangement in the equilibrium of the beam.

In order to ascertain as much as possible how far the weights of Mr. Robinson might be relied on, and within what limits the errors, arising from some inaccuracy in the weights, were likely to be circumscribed, I requested Mr. Bate of the Poultry to make for me a very accurate brass copy of the one pound British imperial troy weight; and it is but justice to say, that this standard greatly excels in point of workmanship, and the care with which it is preserved from external injury, the rather unsightly appearance of the standard kilogramme of Monsieur Fortin's making.

This troy pound of Mr. Bate was found, by a mean of very many weighings, to be equal to 5759.935 of Mr. Robinson's grains, making a difference of 0.065 grains, or $\frac{65}{1000}$ of a grain, or $\frac{1}{88614}$ of the whole. I regret that it lies not within my power to ascertain whether this slight difference lies with Mr. Bate or Mr. Robinson. But it will, however, appear that a difference like this has but small influence on the general result, as far as concerns the kilogramme.

It will scarcely be necessary for me to preface, that, in all these weighings, I constantly employed Borda's method, of avoiding any error arising from the inequality in the length of the arms of the balance beam.

With every precaution, then, I found the authentic brass copy of the kilogramme, brought from Paris by Mr. Van Swinden, adjusted by the celebrated commission of weights and measures, and made by M. Fortin, to be equal to

1st weighing		15432.350	
2d	"	15432.320	
3d	"	15432.305	
4th	"	15432.204	of Mr. Robinson's British imperial troy grains.
5th	"	15432.290	
6th	"	15432.265	
Mean		15432.295	

However, as I consider the last weighings much m
rate than the first, I have no hesitation in adopting 1
of Mr. Robinson's grains as a very near approxin
the value of Mr. Van Swinden's brass standard kil
From this we have the British Imperial troy pound
373.244 grammes; the British troy ounce, 31.1037 g
the British troy grain, 0.0648 grammes; and the kil
equal to 2 lbs. 0 oz. 3 dwt. 0.265 gr.

Besides this duplicate of the original kilogramme,
opportunity of examining many standards of the san
weight, said to be very accurate. They are the follo

2. A kilogramme sent by the French administrati
then imperial mint, at the time when Napoleon inc
this country in his empire. This weight was intenc
used as a standard in the mint at Utrecht; it was
M. Gandolfi, *balancier de la Monnaie de Paris*,
called *kilogramme modèle*. It was stamped V. G., t
of the maker, and M., signifying *modèle*.

3. A kilogramme, also sent by the French impe
administration in Napoleon's time, to the mint at
made also by Gandolfi, and marked V. G.

4. A kilogramme in brass, made, it is said, with gr
racy by M. Fortin in Paris, and belonging to the m
the interior of this country.

5. A kilogramme in brass, being the standard ma
at present in the mint at Utrecht, and made in thi
by T. A. Nagel, inspector of weights and measures.

6. A kilogramme, by the same maker, belongi
Royal Institute of Holland, and marked in consequ
the private stamp of that body.

7. A kilogramme, by the same maker, belongin
and stated to be made with great care. It is marked

8. A kilogramme, by the same, belonging to me,
to be very accurate.

These different kilogrammes, expressed in Mr. R
troy grains, gave the results contained in this Table:

No.	By whom the Kilogramme made.	To whom belonging.	Value in Mr. Robinson's British Troy Grains.	Difference with Mr. V. Swinden's Standard.
1.	Fortin . . .	Mr. Van Swinden's Standard . .	15432.265	0.
2.	Gandolfi, V. G. and M.	Mint at Utrecht .	15432.730	0.465 gr.
3.	Idem, V. G. . .	Idem . .	15433.752	1.487
4.	Fortin Modèle . .	Ministry of the interior	15432.752	0.487
5.	T. A. Nagel, Amsterdam . .	Mint Standard at Utrecht . .	15432.920	0.655
6.	Idem . . .	Royal Institute of the Netherlands	15432.985	0.720
7.	Idem, No. 2. . .	Dr. Moll . .	15433.42	1.155
8.	Idem . . .	Idem . .	15434.91	2.645

It is said that Dr. Kelly found a copy of a very accurate standard kilogramme, sent over from France to the British Mint, to weigh 15.433 British troy grains, but as I do not possess the *Universal Cambist*, I take this information at second hand.

Dr. Weber of Berlin was furnished with a brass standard of the British imperial troy pound, procured by Professor Schumacher, with one of Mr. Robinson's balances, and with a platinum kilogramme belonging to the Prussian government. He found 1 lb. British troy, or 5760 grains=373.2484 grammes *: this makes the kilogramme equal to 15432.08222 grains, leaving a difference with Mr. Van Swinden's kilogramme of 0.183 grains. It is natural and just to suppose that Dr. Weber paid due regard to the circumstance, that the *platina* kilogramme, if equal to the weight of the two platina kilogrammes kept at Paris, one in the Archives Nationales, and

* Poggendorff's Annalen der Physik und Chemie, 1830, vol. xviii., No. 4, p. 608.

the other at the observatory, will be of unequal weig correct *brass* kilogramme weighed *in air* *.

The result of all these experiments is, that we differences between weights, which ought to have bee which altogether appear intolerable, and there rer obscurity about the real value of these weights, wh difficult to account for. Yet, if we are to look to the rative weights of the kilogramme, and the British tro as deduced, not by actual weighing, but by comput shall find differences still greater and more bewilderin

1st. In 1769, Tillet, the French academician, de by actual weighing, the relation of the British troy p French *poids de marc* †; hence by the known proporti poids de marc to the kilogramme, as determined by Gineau, we may compute the value of the kilogr British troy grains.

2nd. In Dr. Young's Lectures on 'Natural Philos a calculation of the comparative weight of the kil and British troy, from the known ratio of the m British yards, and the experiments of M. Lefevre and Sir George Shuckburgh on the weight of a cubic ter and a British inch of water at a certain temperatu

3rd. In the Appendix to the third report of the commissioners for weights and measures, the result is a similar computation drawn from the same source repetition of Sir George Shuckburgh's experiments by Kater §.

4th. Mr. Mathieu of the Paris Observatory, compu French and English experiments the value of the kil in British troy grains ||.

From all these operations the following different of the kilogramme to British troy grains was const which the value of Mr. Van Swinden's standard kil has been prefixed. We may perhaps, all at once, the result deduced from Tillet's operations, as prol

* Base du système métrique décimal, tome iii., p. 644.

† Tillet, Mém. de l'Acad. Roy. des Sciences, 1769, p. 384.

‡ Dr. Young's Lectures on Natural Philosophy, vol. ii., p. 1

§ Annals of Philosophy, 1821, New Series, vol. ii., p. 154.

|| Annuaire du Bureau des Longitudes, 1829, p. 59.

scales and weights used in his time could hardly possess that accuracy and sensibility which are required at present.

No.	By whom the Calculations were made.	Kilogramme weighs in British Troy Grains	Differences with V. Swinden's Standard.
	Mr. Van Swinden's Standard.	15432.265	—
1.	Tillet . . . . . / Van Swinden . . .	15445.716	13.451 grs.
2.	Sir George Shuckburgh . / Dr. Young . . .	15444.03	11.765
3.	Sir George Shuckburgh / Mr. Fletcher, Captain Kater	15444.0	7.735
4.	Mr. Mathieu. . . .	15438.355	6.090

Differences amounting to such an enormous extent are truly appalling, and some secret cause must exist why all the comparative values of the two weights, as found by calculation, are so widely different from what they are found by actual ponderation.

Next, all the kilogrammes, which were actually weighed, differ amongst each other, and also from that standard which was made under the immediate inspection of the contrivers of the metrical system. Of all these, the platina kilogramme used by Dr. Weber is the lightest; then follows the brass standard of Mr. Van Swinden, and all the rest are considerably heavier. It might be argued that those made in this country were adjusted with no great nicety; but what are we to say or to think of the differences between those made by Fortin and by Monsieur Gandolfi, *balancier de la Monnaie de Paris?* Some gross error must undoubtedly lay concealed in some parts of the operations, and strange suspicions as to the source of these errors must arise in the minds of any one who looks into the matter; perhaps it is not irrational to suppose that errors lurk, where the least light is thrown in.

We are in full possession of the facts, on which the determination of the metre rests, but we are far from having such

full and complete information as to the means by v kilogramme was determined. The coincidence of th ments on the length of the pendulum made by Capta and Monsieur Biot affords sufficient evidence of the with which these operations were conducted; but kilogramme was come by, has never been very sati explained. Indeed, M. Delambre, in the 'Base du Métrique,' having explained how the metre was det says, that the account of the fixation of the unit c ought to have followed; but that the multifarious a of Mr. Lefevre Gineau, as professor, as an insp studies, and finally as a member of the legislative c did not allow him sufficient leisure to lay the finishing the arrangement of his papers, although the plates v graved long ago. In consequence, not the particular experiments, but the account of what was done, as d by Tralles, is given as it was read before the Institu this is a report of the experiments, not the descriptio experiments themselves; we have the shadow, not t itself, and we are entirely in the dark as to the parti so interesting and intricate an operation as the deter of the unit of weight.

It must be observed that the operations by which t gramme was determined were closed in 1799, and Delambre's evidence as to the supineness of M. Lefevre is given in 1810. Thus, in eleven years M. Lefevre co spare time for a work so important, and on which his s reputation was chiefly to rest: for it must be recollect great as the merit of M. Lefevre as a legislator an inspector of the University possibly might have been, not attached his name to any other scientific operatic the determination of the kilogramme. All this would c have been entirely different if the ingenious and la Borda, who was the soul of all the operations for dete the metre and kilogramme, had not been untimely s away. But, taking the matter as it stands now, it n confessed, that we know very little of the means by wh unit of weight was determined.

But whatever is the degree of uncertainty prevailing

the real value of the kilogramme, at least the different duplicates of this weight should have been equal amongst each other, which, however, is far from being the case. I am sorry to add, that some such variation is, to a certain extent, prevailing as to the British troy pound. I have noticed already, that there is some slight difference between Mr. Bate's troy pound and Mr. Robinson's 5760 grains, but there are instances of much greater differences.

In 1818 the British consul at Rotterdam applied to the general masters of the mint at Utrecht for a standard copy of that weight, which, at that time, was used in the Mint of this country. In consequence, a standard copy of the Dutch troy pound was prepared and forwarded to the British consul, and he was requested to procure in return a standard of the British troy pound. Agreeably to this, two copies of a brass standard British troy pound were adjusted at the Mint in London, and sent to this country, together with a certificate from a Mr. Field of the London Mint. I have examined both these copies of the British troy pound, and one is actually in my possession, whilst the other is kept at the Mint office in this city. Upon each of these brass troy pounds the following inscription is engraved, which stamps them, as it were, with a character of officiality:

BRITISH TROY POUND
= 5760 GRAINS.
FROM
HIS MAJESTY'S MINT.

Notwithstanding this certificate of authenticity, and a paper belonging to them, in which a Mr. Field, an officer, as it would appear, of the Mint, asserts that they were carefully adjusted, both these weights are unequal to each other, and to the 5760 of Mr. Robinson's grains.

That which is at present kept in the Mint Office of Utrecht weighs	5758.57 gr
The second, now in my possession . . .	5758.40
Whilst Mr. Bate's imperial troy pound holds . . .	5759.93

It is exceedingly vexing to see weights adjusted in Mint of England, and on which expense has not been spa differing more than $\frac{1}{10}$ of a grain from each other. Fu more, the difference of these weights, Mr. Robinson's

* The sum of 5*l*. 5*s*. was charged for making and adjusting these w

and Mr. Bate's pound, is so great, that we cannot h
ing but that there must exist some notable differenc
the Mint standard of troy weight and that according
both Mr. Robinson and Mr. Bate made their copies.

By an act of the 5th George IV., the standard o
the custody of the Clerk of the House of Commo
clared standard of the British troy weight. This sta
made or adjusted in 1758 by Mr. Harris, then Ass
of the Mint. We cannot, therefore, but admit tl
1758, an enormous and unaccountable change took
tween the troy weight as used in the Mint of En
that copy which remained unaltered in the custod
of the principal officers of the House of Commons.

Although the weights formerly in use in this cou
been abolished and replaced by the French kilogr
may not perhaps be altogether superfluous to comp
old weights with the British troy and avoirdupois,
blished by the act of 17th June, 1824, 5th George IV

Formerly there were three different sorts of Dutcl
in use in Holland.

1. The Dutch troy weight, differing essentially
British troy. It was used in the concerns of the Mi
weighing of gold and silver, and also for medical,
ceutical, and philosophical purposes. In the pro
Friesland it was the general weight for commercial
tions, and no other weight was employed there.

2. The Amsterdam commercial weight, genera
throughout the country for all commercial concerns o
scale; it was originally derived from troy weight.

3. In shops, and for the retail trade, a light we
almost generally used, except in Amsterdam. It
employed, in some cases, in the iron trade. It is c
the Antwerp weight.

The general opinion is that troy weight derives its na
the city of *Troyes in Champagne*, in which place it
that very heavy weight was used in old times. It wou
however, that there is no document in existence at pr
the records of that town, which is calculated to th
additional light on the history of the introduction

weight, which, with different modifications, has been adopted both in Holland and England.

In the fifteenth century, troy weight was used in Amsterdam; and in 1520, the Emperor Charles V., then sovereign of the Low Countries, ordered that the mint-masters in the several provinces should adjust their weights to the standard of troy weight kept in the offices of the several courts of accounts (*cours des comptes*) in the different provinces. The old standard of this weight, even now existing in the Hague, has been adjusted in 1554 to the standard of the court of accounts of Brabant at Brussels. The same regulations as to the use of troy weight have been reinforced by the statute of the Earl of Leycester in 1586, and by that of the States-General of 1606. However, the troy weight in use in the Seven United Provinces for upwards of a century down to the time of its being abolished in 1819, was somewhat different from the old standards; but these matters, being of a local nature, do not require to be stated here in all their particulars.

Mr. Van Swinden, before he set out for Paris in 1798, to attend the meetings of the great commission of weights and measures, procured a copy of the Dutch troy weight, as it had been used for more than a century, and had it adjusted with great care. This duplicate Dutch troy weight he took to Paris, and by its means, he, M. Aeneae, the second commissioner, and M. Lefevre Gineau investigated and determined, with all the accuracy which they could, the relative proportion between the Dutch troy weight and the kilogramme. This standard, religiously preserved, came also into my possession, and I have several times weighed it against Mr. Robinson's British troy grains, with all the attention which I could master. Accordingly the weight of

One brass pound of Dutch troy weight is 7594.975 grains of Mr. Robinson.
= 15 oz. 16 dwt. 10.975 grs.
= 15 oz. 6 drachms, 1 scruple, 14.975 grs.

And one pound Dutch troy = 0.7583961764193 lb. British troy.
One ounce Dutch troy .... = 474.6856 grs. British troy.
One grain Dutch troy..... = 0.989 gr. British troy.
One grain British troy.... = 1.0112 Dutch troy.

And finally, assuming the kilogramme to be equal to 15432.265 of Mr. Robinson's grains, we have one pound Dutch troy weight equal to 492.14907857 grammes.

The Dutch troy weight, as used for weighing bulli precious metals in general, has sixteen ounces, but f and pharmaceutical purposes, there are twelve ou pound. But the ounces are the same in both cases, tain 480 grains.

For mint purposes and the weighing of bullion, th of the Dutch troy was as follows:

1 lb. Dutch troy=2 merks,
1 merk=8 ounces
1 ounce=20 sterlings (English)=
1 sterling=32 aas.
Thus 1 lb. Dutch troy=10240 aas=7680 Dutch troy grains.
=7594.975 British troy grains.

For medical and pharmaceutic use, the division of troy ounce was as follows:

One Dutch troy ounce=8 drachms=480 grains.
=1 drachm = 3 scruples=60 grains.
1 scruple=20 grains.

As the British avoirdupois contains 7000 grain troy, it is a matter of computation to deduce from t relation of the Dutch weight to English avoirdupois.

1 lb. avoirdupois=0.92166202 lb.
=14 oz. 14 sterling, 29.8181 aas. } Dutch t
=7078.3643 grains.

and inversely,

1 lb. Dutch troy =1.0849964 lb. avoirdupois.
=1 lb. 1 oz. 5.75908 drachms avoirdupoi

In the golden old times of trade, the magistrates o dam were anxious that the commercial weight of should be heavier than that used in other tradin Therefore it was established by law, that the Amsterd mercial pound should be 40 aas in the pound hea troy weight. In consequence of this regulation, we following relations between British and Hollands co weights.

1 lb. avoirdupois=0.9180754 lb. Amsterdam weight.
=14 oz. 1.37841 lood Amsterdam weight
1 lb. Amsterdam=1.0892347 lb avoirdupois.

* The Amsterdam pound is divided into thirty-two parts called *loode* I know of no English corresponding word.

An English hundredweight, or 112lb. avoirdupois=102 Amsterdam weight.

How far the use of heavier weight than our neig may be beneficial in a commercial point of view, and w such attempts may lead to any practical result, is not f to investigate.

I. ON A REMARKABLE CASE OF CORYZA PHLEGMATIC

II. ON THE DIRECT FORMATION OF OXIDE OF IODINI AND IODOUS ACID.

III. ON NITROGEN IN NATURAL WATERS.

By the CAVALIERE LUIGI SEMENTINI, of Naples, M.R.I. &c.

In a Letter addressed to the Secretary of the Royal Institution.

I. CORYZA PHLEGMATICA.—The following account of a uncommon disease, which has recently come under my observation and cure, may not be unacceptable. A similar affection has been called by Sauvages and Borsieri, *Coryza Phlegmatica* or *Phlegmatoragia.*

The patient, who was about fifty years old, abounding in humours, and of a sanguine temperament, after a strong fit of sneezing, was suddenly affected with a constant running from the right nostril of a clear liquid, in large drops, to the number of twenty-five in a minute, and of twelve at the least; but the diminished number occurred only during the hour after dinner. The discharge increased to the larger number of twenty-five drops per minute in the evening, and continued without interruption during the whole course of the night. Hence the patient was obliged to take his rest in an uneasy position, with his head inclined forward, without which precaution he would have been suffocated. In walking, on the contrary, he went with his head erect, in order that the liquid might fall into the fauces, from whence he discharged it by large mouthfuls, thus avoiding the disgust of having his face and clothes continually soiled.

The fluid in question had a strong saline taste, and the patient was obliged to muffle up his nose during meals, in

order that it might not mix with his food. Neverth was not afflicted with head-ache, nor with any uneas tion in the fore-part of the head, nor indeed with ar ordinary phenomena accompanying such diseases. lost a little flesh, and in other respects enjoyed perfec

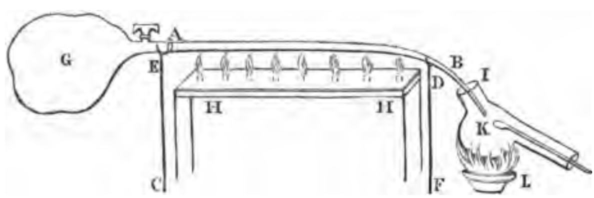
This disease, which, in my time at least, has not bee to occur in Naples, is described by the celebrated Mo his fourteenth Medico-Anatomical Letter, where he the diseases of the head. But the case he describes o tian lady, who was affected with it, and cured by hin from that which came under my observation, in that t tity of the fluid discharged was not above one half, it continued for near a year, while in my patient it c only four months; and because the health of the lad sensibly affected by it, as to endanger her life.

The disease was for a long time treated by me in th tomed derivative method of foot-baths, blisters, s purgatives, &c., but in vain. Afterwards, remembe the afflicted person was subject to the gout, thoug anomalous and irregular form, I was of opinion that of the gouty humour might have affected the olfactor and the result of my experience being that no me more effective than James's antimonial powders to re disease to a state of regularity, and therefore prc remove this humour from the unaccustomed seat it I subjected the patient to a course of that medicine. end of four days, his head, which had always been pain, became slightly affected; and at length, afte perspirations, the disease, which had come suddenly, a manner quite as sudden.

The fluid discharged, although transparent and lim it first issued from the nostril, soon became turbid, anc a light yellow colour, depositing a flocculent animal s In its composition, beside the muriate and phosphate I found a considerable quantity of urate of soda, esp the flocculent deposit: there was scarcely any free sc

II. Iodine and Oxygen.—I presume that you are acquainted with my experiments on the combinations of iodine constituting the oxide of iodine and the iodous acid*. This second substance was obtained by triturating the chlorate of potassa with iodine, and heating the mixture in a distillatory apparatus. But Woehler having objected that this substance was a chloride of iodine, and not a distinct acid, I was obliged to vary the experiments, the results of which I am anxious to communicate to the members of the Royal Institution without delay.

In replying to Woehler, I undertake to prove that, on all occasions when iodine is brought in contact with pure oxygen gas, or even with common atmospheric air, at a *high temperature*, combination takes place, forming first oxide of iodine, and afterwards iodous acid, without the intervention of any other substance. I request your attention to the circumstance of an *elevated temperature*, because, in the French translation of Berzelius's 'Chemistry,' published at Paris, a *low* temperature is mentioned, which would prevent the success of the experiment to which I have the honour to refer you below.



The tube A B of cast brass should be of the diameter of a barometer tube, and ought to terminate at the end B in a capillary aperture. It should be fixed to the supports E C, D F. At the extremity A, a bladder G with a stop-cock should be fixed on, which is to be filled with oxygen gas, or even with mere air. The oblong spirit-lamp H H is to be so placed that the flame of its burners may act upon the whole length of the brass tube A B. The extremity B of the tube is to enter the tubular opening of the empty retort K, into which it is to be fixed by luting. Under the bulb of the retort is to be placed the large spirit-lamp L.

* Quarterly Journal of Science, vol. xvii. 381, and vol. xxiii. 477.

The apparatus thus disposed, the burners of the lar being lighted, and that of the lamp L placed under the when the tube and the retort are very hot, the bladde end of the tube is to be pressed by an assistant, for gas to issue from the capillary end of the tube, the menter, at the same time, introducing a spoonfull c into the neck of the retort, so that the bowl of the spo come immediately under the capillary opening of the tu In this operation the iodine is soon raised into vapou coming immediately into contact with the heated oxy combines with it, and assumes the form of an amber-c vapour, which condensing in the neck of the retort, be dense oily fluid, which is the oxide of iodine, first discov me. By continuing the jet of oxygen gas, I have upon tw sions obtained iodous acid, but it is incumbent on me t confess, that in following up the experiments lately I h had the same result, without being able to account cause of such irregularity.

Yet, nevertheless, by these or by any other mean iodine is made to combine with oxygen at a high temp the oxide of iodine is constantly obtained. The follo a new experiment by which it may be obtained in co able quantities without complicated apparatus, and employment of such simple means that no doubt or dis can arise as to its nature.

The deutoxide of barium is to be triturated for son with iodine, in such proportions that the iodine is in and that the mixture acquires a blackish colour. Thi der is afterwards to be introduced into a small retort long neck, and the heat of a large spirit-lamp applied. violet-coloured vapour which first appears is soon suc by the yellow vapours of the oxide of iodine, which sul collects in the neck of the retort, and ultimately drop it. The same result is obtained with the protoxide of b but the effects are less sensible and the product not so abu in operating with the protoxide as well as the deutoxide, gas is always developed.

When the substances employed are free from water, t lowing are the properties by which the oxide of iodine tinguished;—

1. It has a yellow amber colour.

2. It is soluble in water and in alcohol, forming coloured solutions.

3. It is thick like oil, and it is frequently necessary to heat the neck of the retort in order to collect it, after which it remains more fluid. The excess of iodine frequently causes the violet-coloured vapour to pass over with the oxide of iodine; which, however, does not alter the result of the operation, as the two substances do not mix. If the iodine and the oxide of barium are not perfectly dry, the oxide of iodine is much more fluid. It changes the colour of blue litmus paper to an emerald green.

Oxygen is so slightly united to iodine, that the simple contact of a combustible body at any temperature suffices to disunite them, and the iodine is separated with all its properties. Whether the oxide be concentrated or weak, its decomposition is effected by mere contact, even with a piece of white card, which soon becomes covered with a black stratum of iodine.

When the oxide is in its state of greatest density, it ignites phosphorus and potassium by simple contact.

By such evident proofs, therefore, the formation of the oxide of iodine is demonstrated; that of iodous acid becomes very simple and clear, as I shall have the honour of communicating in a future paper in which I purpose to treat of the iodites.

The few facts above stated have not yet been published, with the exception of that which relates to the action of air or oxygen gas with iodine at a high temperature, and I communicate them to the Royal Institution as a mark of my respect, in order that they may be published in the Journal, should they be thought worthy of it.

III. Nitrogen in the Waters of Castellamare—A commission, of which I am a member, has undertaken the analysis of the mineral waters of Castellamare, but owing to my indisposition its labours are not yet finished.

Reserving to myself the pleasure of communicating to you the results when completed, I will merely now state that their

composition is very similar to that of the waters of Sp
season the concourse of English visiters to those wat
siderable: it may not, therefore, be uninteresting
cal men to learn, that these waters contain azote in
able quantities:—a fact not new in chemistry, b
common, and of great importance.

ON THE DIRECTION OF THE RADICLE AND C DURING THE VEGETATION OF SEEDS.

By THOMAS ANDREW KNIGHT, Esq., F.R.
President of the Horticultural Society, &c.

IN the 'Quarterly Journal of Science' of the last which publication I regard the 'Journal of the Ro tution' to be, in some degree, a continuation,) a comm made by M. Poiteau to the 'Société d'Horticulture' is noticed; in which that writer considers himsel totally refuted and annihilated my hypothesis resp descent of the radicle and ascent of the germ of ge seeds, which was published in the 'Philosophical Tra of 1806. M. Poiteau proceeds to slay the slain; having, as he supposes, proved that my inference physiologically correct, he goes on to say that the at all physiological, and that 'Il convient donc que nistes raient l'expérience de MM. Knight et Dut catalogue des expériences de physiologie végétale,' ha viously determined that 'il n'y a rien de physiologi l'expérience de M. Knight.'

I had previously seen several attempts to refute thesis; but, in all these, it was either misunderstood misrepresented. I must give M. Poiteau, however, of having fully understood and fairly represented m thesis; and the only grounds upon which I can obj conclusions are that, as far as they are connected w table physiology, all his premises and all his infer false; and that if all his premises had been perfectly his inferences would have been totally erroneous.

M. Poiteau attached pieces of metal, of the for

seeds of gourds, to the circumference of wheels, similar to those upon which I bound germinating seeds, each piece of metal being perforated near its more pointed and smaller end, and fixed to the wheel by a pivot, round which it was left at liberty to revolve. When these pieces of metal were subjected to the operation of centrifugal force, their heavier ends necessarily receded from the axis of rotation, and the lighter ends were necessarily made to point towards it. M. Poiteau conceives that nothing more occurred in my experiments and those of M. Dutrochet, who repeated the same experiments with very superior machinery, and with the same results; and that the sole cause why the germs approached the axis of rotation was, that their specific gravity is not greater than one-third that of the radicles. I was not so fortunate as to be able to comprehend this; and though I gave M. Poiteau full credit for accuracy respecting the different degrees of specific gravity of the substance of the radicles and germs, I thought the fact a very extraordinary one. I therefore planted a couple of dozen seeds of the plant (*phaseolus vulgaris*) which was the subject of M. Poiteau's experiment, and I then discovered, to my astonishment, I confess, that the radicles, instead of possessing a degree of specific gravity three times greater than that of the germs, were really the lighter body of the two, the germs having all sunk to the bottom of the same vessel of water in which all the radicles rose to the surface. I repeated this experiment several times, and always with the same result, having detached both the radicles and germs from their cotyledons as soon as the germs became visible above the surface of the ground.

The seeds in my experiments were bound firmly to the circumference of the wheels, instead of being, as M. Poiteau's pieces of metal were, left at liberty to revolve upon pivots; and the direction taken by the radicles and germs of seeds is totally independent of each other. The germ is never made to deviate, in any degree, from its perpendicular line of growth upwards by any obstacle which the radicle meets with in its descent; nor is the direction of the radicle ever influenced, in any degree, by that taken by the germ. If the seed of a peach, or pear, or other tree which nature intended to support

itself be planted, its germ will be seen to incline towa
point from which it receives most light; whilst the germ
ing from the seed of ivy, or other plant which nature i
to rely upon the support of some other body, will rece
light and seek the shade; but the radicles of all will b
to proceed alike perpendicularly downwards; and, th
if the specific gravity of the radicles had exceeded tha
germs to the extent conceived by M. Poiteau, that
stance, as the seeds in my experiments were bound fi
the circumference of the wheels, could not, in any
whatever, have caused the germs in their growth t
approached their axis of rotation. M. Poiteau must,
fore, allow me to consider his production as a succe
blunders from beginning to end.

I wished M. Dutrochet to refute M. Poiteau's hyp
but he seemed to think that M. Poiteau's errors w
obvious to every reader before he could publish his ex
of them; and I should have agreed in opinion with
trochet if M. Poiteau's communication, instead of his a
tion, had been published in the 'Quarterly Journal of S
but, as it was not, and as the knowledge of the influ
gravitation upon the moving fluids of plants, and
quently upon their growth, forms, and produce, is impo
the scientific gardener; and as I had the honour to
from the Royal Society Sir Godfrey Copley's medal
memoir which gave a statement of my facts and hypot
have thought it proper to shew that M. Poiteau's conc
are not quite so accurate and unquestionable as he ap
have imagined them to be.

Downton, July 8, 1831.

ON DISINFECTION AND THE PRACTICE OF QUARANTINE; WITH SOME REMARKS AND COMMUNICATIONS RELATIVE TO CONTAGIOUS DISEASE, AND ESPECIALLY THE CHOLERA.

By ANDREW URE, M.D., F.R.S., &c. &c.

THE remarkable power of chlorine, and of its officinal compounds, chloride of lime and soda, in decomposing and destroying the fetid effluvia of animal and vegetable bodies in a state of putrefaction, has been so long known, has been verified in so many instances, and is susceptible of such direct demonstration, as to be beyond the cavils of medical pyrrhonism in its most wanton mood. That these effluvia are capable of making morbific impressions on the living body, is also placed beyond any reasonable doubt, not only by the sickness they instantly occasion, but by the many recorded cases of fevers of a putrid or low typhoid type, brought on by incautious exposure to masses of animal matter far advanced in putrefaction. The power of such matter to produce fevers by inoculation, has been often fatally exemplified in the dissecting schools; and the power of a lotion of chloride of lime or soda to counteract danger from such inoculation, is now equally well ascertained. In a letter just received from my son, at present House-Surgeon of the Glasgow Royal Infirmary, he says, 'Having performed several *post mortem* dissections of persons who have died from malignant fevers, dysentery with extensive ulceration of the mucous membrane of the large intestines, peritonitis with purulent effusion into the abdomen, hectic from suppuration, gangrene, &c., I have never suffered the slightest inconvenience. Yet these are the cases in which that peculiar animal poison is especially generated which has occasionally proved fatal to the demonstrator of disease. I attribute the immunity I have enjoyed, in a great measure, to my washing my hands immediately after each inspection with the chloro-sodaic liquor of Labarraque; this I prefer to the solution of chloride of lime, as it is not so apt to injure the skin.

'A young gentleman, who acted as my colleague during

part of last winter, but who did not adopt the above pre
having imbibed through a minute breach of surface
little finger a portion of this virus, was in a few hour
after attacked with acute inflammation of the absorben
arm, accompanied with high symptomatic fever, whi
fined him to his bed for many weeks, and required t
powerful antiphlogistic measures to subdue the infla
symptoms. I could cite instances of my predecessors
suffered from the same cause, but I deem it unnece
the fact is indisputable.'

A mournful example of the danger of putrefactive
occurred a considerable time ago in the north of S
Two young medical men, desirous of examining a bod
had been interred without dissection, in consequenc
prejudices of the relations of the deceased, went in a v
night to exhume it, but having mistaken the grave, l
a coffin replete with such noisome corruption, that th
men instantly sickened with the fetor, were hardly ab
home, where they forthwith took to bed with sym
malignant fever, and died. MM. Orfila, Leseure, Ge
Hennelle, were employed, about seven years ago, in
examine the body of an individual who was supposed
been poisoned, and who had been dead and buried fo
a month. Had they rashly proceeded to the inspecti
would most probably have fallen victims to their impr
but the smell was intolerable, and the body could h
approached; they had, therefore, recourse to chloride
sprinkling a solution of it over the putrid corpse, wh
duced, after a few aspersions, such a wonderful effect,
nauseous effluvia were instantly quenched, and the d
was performed with comparative comfort.

Chloride of lime has been repeatedly used since wi
efficacy in similar cases; it has become a famili
putrescent agent in the anatomical theatre, and has l
plied to destroy the stench of bilge-water and commo
with unfailing efficacy. Its operation on fish so muc
as to be hardly fit for the table I have myself repeate
and I have found that a dish of such fish cleaned an
up, by immersion in a dilute solution of the chloride

minutes, loses the dark colour at the bone and all offensive scent; and after being washed in water, when boiled it possesses the curdy firmness, sea-air flavour, and taste of newly caught fish. An ounce of good chloride of lime is sufficient to sweeten a very large dish.

The phenomena of putrefactive fermentation seem to show that the fetor resides in certain hydrogenated compounds, containing carbon, sulphur, phosphorus, azote, &c.; for gaseous matter of this kind is eventually disengaged in the larger cavities of the trunk, as well as in the cellular tissue, causing a general intumescence. There is every probability, likewise, that the diffusible fomes of contagious disease resides in some analogous compounds, but of so subtle a nature as hitherto to have baffled every effort of chemistry to collect and analyze. The same thing may be said of the miasmata of marshes. The infectious virus of plague, small-pox, and putrid bodies, resembles in some measure the poisonous secretion of venomous reptiles, and is of a more durable composition and less volatile (so to speak) than the effluvia of typhus, scarlatina, and measles. We can therefore easily understand why an agency capable of decomposing the former morbific powers, may be feeble to grapple with the latter, embodied as they are in a too palpable humour or a solid crust.

Guyton Morveau appears to have been the first man of science who directed the resources of pneumatic chemistry in a regular manner to the purpose of disinfection. The Cathedral of Dijon had been for several years infested with a febrile fomes or miasma, which occasioned fever in many of its pious visitants, and it had become in consequence nearly deserted as a place of worship. Being then (1774) Professor of Chemistry in the Academy of Dijon, M. Guyton was naturally induced to exercise his science in expurgating the air of the church. He accordingly filled the whole capacity of the building with muriatic acid gas, disengaged from a mixture of salt and sulphuric acid distributed in a number of stone-ware dishes. The doors and windows were kept close for two or three days, to prevent the dissipation of the acid fumes. At the end of this period a free ventilation was given by opening the doors and windows, after which the church was found

to be deprived of its unpleasant smell and unwhol effluvia.

In 1796, Dr. Carmichael Smith applied the fumes of acid, disengaged from nitre by sulphuric acid, to the fection of a ship's hospital, for which he received a co able parliamentary reward.

Since that time the progress of chemical research has us more fully acquainted with the intense affinity which between chlorine and all hydrogenated compounds, ar the resulting anti-putrescent quality of chloride of Hence chlorine has naturally come to be regarded as th energetic antiloimic agent. In this respect, likewise, th of its introduction belongs to M. Guyton, who recomr medical men, nurses, and other attendants on contagio ease, to carry about with them phials containing mar and muriatic acid, and to open the glass stopper fro to time in situations replete with infectious effluvia, ir that the chlorine exhalations might decompose them, a serve a healthy atmosphere for respiration. In the se the present paper, facts will be adduced apparently the efficacy of this antidote to the contagion of cholera.

As gaseous chlorine in the state in which it is evolve muriatic acid and manganese, has been thought to be tc centrated for diffusing in apartments occupied by th recourse has been had in a great variety of cases to the lations that spontaneously rise from chloride of lime e in an extensive surface, either in its pulverulent form solved in water. It is true, indeed, that under both o forms the chloride exhales its peculiar odour, but it gi no appreciable or operative portion of gas, and ins losing, it gains weight. I have suspended a piece c litmus paper within three inches of good chloride of lin stoppered phial for upwards of an hour, without its blanched; nay, the paper retained much of its colour end of twenty-four hours. As the paper would have l white in a few minutes by the admission to the phial tenth of a cubic inch of chlorine gas, it is obvious tha that minute volume was not disengaged from the cl which amounted to nearly 500 grains. But by the ag

muriatic acid, that quantity of the said chloride would have evolved about 145 grains, or 190 cubic inches of pure gas. I may remark, that few samples of the bleaching-powder found in the market are impregnated like the above with fully 29 per cent. of chlorine, and the stuff retailed in many shops under that name seldom contains more than 16 per cent. As for the liquid chloride of lime, the two-shilling bottles occasionally possess no more virtue than would be found in two-pennyworth of Messrs. Tennant's dry bleaching salt. Nothing, therefore, can exceed in absurdity the fashionable nostrum for disinfecting apartments charged with contagious fomes, by placing in them one or more saucers filled with chloride of lime. To place this dangerous fallacy in the plainest light, I need merely state that moist litmus paper may be hung for a day a very few inches above such a saucer without perceptibly losing colour; whereas the affusion of a few drops of muriatic acid on the same chloride, even after the above period, will instantly blanch the suspended paper.

It has been supposed that the carbonic acid present in the atmosphere displaces the chlorine from the lime; but how slowly and insignificantly the preceding experiment may show. The following facts have been long before the medico-chemical world. 'After passing a current of this gas (carbonic acid) for a whole day through the chloride diffused in tepid water, I found the liquid still to possess the power of discharging the colour very readily from litmus paper *.'

Chloride of lime laid out in the air passes rapidly into a deliquescent paste, consisting of muriate of lime, and lime with an obscure displacement of oxygen. If the chloride be surcharged with chlorine, it speedily gives off the excess and becomes commercial chloride. The best manufacturers, aware of this circumstance, never push the impregnation beyond a certain pitch; in which state the chloride does not spontaneously emit in the air one-thousandth part of its condensed chlorine. To pretend, therefore, to suffocate the hydra of contagion by subjecting it to the simple smell of chloride of lime

* On the Manufacture and Composition of Chloride of Lime, by Dr. Ure, Quarterly Journal of Science and the Arts, for July, 1822.

in a saucer, is just such a mockery as it would be to ı the famished stomach by the smell of a cook-shop. Th‹ effluvia of a pestilence must be combated by more eı means; they must be environed with an atmosphere ‹ rine adequate to effect their destruction. Every thiı of this consummation is paltering with the safety, not o individuals, but possibly of a nation.

But I shall be asked, whether chlorine gas can be ‹ through the air of a chamber without injuring the l living beings, as well as the furniture and goods? I yes, when it is distributed on philosophical principles. might ask the medical practitioner, in return, whetl corrosive sulphuric and nitric acids may be admiı internally? Yes, he would be ready to reply, wheı ciently diluted; and the same answer will serve for cl I have been a frequent inmate of manufactories of c of lime on the greatest scale, and I have occasionally the atmosphere, in certain departments of the works impregnated, in a sensible degree, with chlorine gas. litmus paper would have speedily lost its colour in sı atmosphere, although dyed woollen and calico stuffs, dry state, suffered no perceptible change. The workm‹ habitually respired this chlorified air experienced n effects on their health, nor, indeed, any inconvenience unless an accident befel some joint of their apparatus. facts prove the safety of immersion in chlorine larg‹ luted with air, yet still strong enough to blanch moist paper; which may be regarded as a satisfactory criterioı activity when directed against contagion.

In applying chlorine gas to apartments, we should bear in mind, that it is one of the heaviest of elastic and therefore it tends to occupy the lower region in pref to the upper. If, in the little cave near Naples, call *Grotto dei Cani*, the carbonic acid adheres closely floor, so that, by rising hardly above the knee, a maı tinues to breathe in perfect ease, unconscious of the pr‹ of his invisible foe, while the dog at his foot is instantly cated, we may judge how much more closely a strat chlorine should adhere—a gas nearly double in den

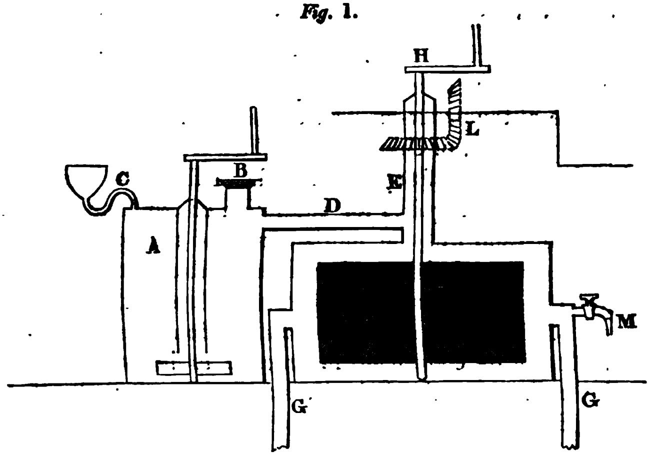
carbonic acid; for air, carbonic acid, and chlorine are in specific gravity respectively as 2, 3, and 5. We need not be told that chlorine, like other gaseous matter, has the faculty of diffusing itself slowly upwards through atmospheric air; but this is only when it has nothing else to do, for when it encounters substances on which it can exercise its pre-eminent affinities, it will combine with them, probably to *their* destruction, and certainly to its own, as an *influential* gas. In proof of this position, I have many experiments to adduce, one of which was exhibited before the Marquess of Lansdowne, Sir Henry Halford, and the whole Board of Health in the Royal College of Physicians, on Friday evening, the 24th of June. Having had the honour, two evenings before, to submit to that Board a plan for disinfecting the cargoes of ships, by distributing dilute chlorine through their holds by the apparatus, figured No. 1, illustrative of this paper, doubts were strongly, and very naturally, expressed by many members of the Board, as to the penetrability of dense bales of hemp, wool, and cotton by the chlorine gas. I was asked, whether I could satisfy them on this head by an experiment; and if so, how soon. I undertook to make the experiment in two days; but an anxiety being shown to have it tried next day, I promised to do my endeavour, with such apparatus as I could command. Accordingly, on the 23rd of June, at four o'clock P.M., miniature bales of hemp, wool, and cotton were made up as dense as possible, the latter two being moreover inclosed in thick canvas bags. They were all put into a tall glass cylinder, open at top, the hemp being placed at the bottom. Chlorine was now introduced through a glass tube, which descended beneath the middle of the jar. In the centre of each parcel, a bit of moist litmus paper was placed before it was bound up. Next evening at nine o'clock, the Board having met again, the little bales were opened, and the papers in their centres were rendered snow-white, clearly proving the penetration. The external fibres of the hemp parcel were so corroded by the chlorine as to be easily torn asunder; while the fibres of the canvas bags, placed above, were not in the slightest degree affected, nor the wool and cotton within them. I have since

found that pure chlorine is pretty quickly absorbed bleached hemp, with the extinction of the peculiar p smell of the gas; but that dilute chlorine blown t among its fibres will blanch moist litmus-paper enclos compact bale, without impairing the tenacity of the h the slightest degree. If merely so much chlorine b duced without agitation, through a tube, into a vessel, a its lower half where a hemp package lies, the gas v spontaneously mount to the upper half, but will conc and expend its energies on the organic fibres below. manner, if chlorine be made to exhale from capsules pla the floor of a still apartment, containing beds and oth niture, the gas will be arrested in its diffusive ascent, a never reach in adequate force the upper walls or ce which the hot effluvia of contagious pyrexiæ (as typhu latina, small-pox, &c.) naturally rise. Should the wall apartment have been recently washed with milk of li gas will be condensed on them; but, if washed with wh no absorption will ensue; for chlorine does not disple bonic acid from lime, nor does it combine with the cal carbonate.

We are thus clearly led to the conclusion that chlor when used as a disinfector, should be considerably dilut air before it is distributed into apartments, in such a and manner as neither to injure furniture nor mercl nor materially to annoy respiration. We must throw view those constitutions indeed which are so delicate tidious as to be intolerant of even the smell of chlorin said aërial mixture should be introduced into the m upper regions, in preference to the lower, and its a should be promoted by propulsion. Moist litmus pap pended in various parts of the chamber, will serve when the chlorine has done its duty.

Figures 1 and 2 (pp. 91 and 93) exhibit two forms of a for disengaging chlorine in regulated quantities, for m with air in any proportion, for blowing it into any space, and for ascertaining the degree of its dilution at a of the operation. Figure 1 has been constructed in th

Fig. 1.



yard at Woolwich, by order of Sir T. Byam Martin, Comptroller of the Navy, from a drawing furnished by me, the copy of that laid before the Board of Health on the 22nd of June. The object of this construction is to show how the cargo of a ship may be imbued with dilute chlorine, without injuring its quality or disturbing its position. Such an easy, quick, and safe immersion in this expurgative gaseous medium will, I presume, be deemed by all persons acquainted with the affinities of this most energetic element, to be a surer safeguard against the importation of contagion in merchandize, than the mere exposure of the goods to the air, as practised under the actual laws of quarantine. At the present crisis of the Russian cholera, the cargoes of hemp, wool, hides, &c., now in the course of arrival on the British shores from the Baltic, and immediately placed under quarantine, are so immense as to require, it has been said on official authority, the decks of ninety-five line of battle-ships for their adequate exposure. Supposing infectious fomes to exist in the merchandize, and the quarantine laws act solely on that presumption, what a formidable mass of contagion will be let loose in our atmosphere, and what a cruel duty is imposed on the sailors immured in the pestilential focus! It appears to me that the danger, expense, trouble, and delay of quarantine may be saved by a just application of the antiloimic virtues of chlorine.

The cask A, *fig*. 1 and 2, is destined to receive the c of lime and muriatic acid. The strong bleaching sal which the Messrs. Tennant supply the London market, for the use of the paper-makers, contains on an average 29 or 30 per cent. of chlorine gas, which it most readil out on the affusion of any liquid acid. I find that one of such chloride requires for saturation of the lime, or (imperial measure) of the muriatic acid of the London of specific gravity 1.160, and evolves a cubic foot and of pure chlorine. This volume diluted with about twelve its bulk of air, is adequate to the disinfection of a small ment. It will be convenient in practice to introduce a into the cask A by the hole B, six or seven pounds chloride, diffused among seven or eight pints of water, an to call forth the chlorine as it is wanted for distributi successive affusions of muriatic acid through the syphon- C, remembering that every pint of the acid will disengage a cubic foot and a half of the chlorine. The gas thus lib will pass along the horizontal pipe D, *fig*. 1, into the s the wide vertical pipe E, and falling down into the venti vessel, will by the motion of its central fanner F be dilute air in any desired measure, according to the velocity rotation. The air of dilution is drawn in at the axis th the open upright pipe E, and the mingled gas is blown through the pipes G G, whence it may be conductec applied whithersoever the operator may choose, upwards, wards, or horizontally, by connexion with wooden or l pipes of communication.

Fig. 1 exhibits an apparatus, which may be got up b ship-carpenter in a day or two with a couple of casks small and the other large. This has been actually do Woolwich. Instead of turning round the axis H of the fa by a horizontal motion of the hand, it may be made to re more conveniently by a vertical motion, provided the end of the axis be furnished with a couple of bevelled-to wheels, placed at right angles to each other, as shown b dotted line L. The stop-cock M, serves to draw off a full of the gaseous mixture, for analysis, by water or mi lime.

Fig. 2.

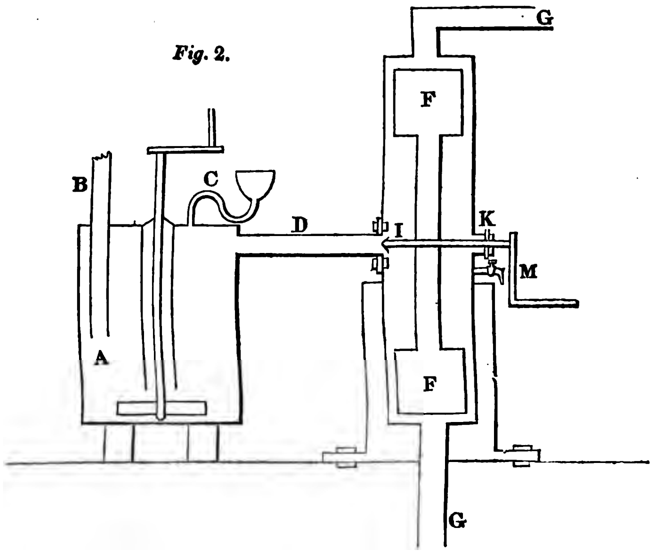


Fig. 2 is a more powerful and convenient form of the same apparatus, for disengaging chlorine, for diluting it with air, and for propelling the mixture along pipes with very considerable velocity, so as to ensure its thorough diffusion among the timbers of a ship, and bales of merchandize, however closely stowed. Every ship of war should be provided with such an apparatus on each of its decks, and a few minutes working of it would sweeten and disinfect, infinitely better than an hour's combustion of gunpowder wetted with vinegar, the *delusive* fumigation at present practised in the navy, under the appropriate title of the *Devil*, the patron of falsehood. The slightest consideration of the gaseous products of burning gunpowder, shows that they can exercise no decomposing influence whatsoever on contagious or fermentable filth, which chlorine unquestionably does. In *fig*. 2, the orifice B of the gas generator is tubular, and rises about 18 inches high, so as to preclude the chance of the relatively dense chlorine overflowing, to the annoyance of the operator. But a few turns of the fanner will draw off the chlorine from A, however quickly it may be evolved. Through the pipe B, the chloride, previously mixed with water, is introduced, and by the syphon-funnel

C, the muriatic acid is to be poured in at proper vals. This syphon may, probably, in the present case pensed with, and the acid may be introduced at B. T engaged gas will flow off along the horizontal pipe D, i fanner F F. When its vanes are made to revolve, atmo air will enter freely by the open pipe B, and passing acı vessel A, will dilute the chlorine to any degree. The gases will be drawn in through the pipe D, and project centrifugal force through any outlet in the circumfer the blowing cylinder F. The horizontal axis of the revolves at one end in the conical bush of a step or sti and at the other in a stuffing-box K. With this app chlorine may be readily propelled in any state of dilu any quantity, and in any direction, through apartments kind. Such an effective application of this anti-put antiloimic element will infallibly exercise an expu influence, no less sweetening to the senses, than salub the system; and ought to banish for ever the sham directed agency of chloride of lime or chlorine, with medical men have so often deceived themselves and th lic*. Wherever chlorine has failed to extinguish in fomes, the operator, and not the chemical agent itself, h in fault. Let us suppose, for example, that the fetid a dissecting-room is to be sweetened; and that this is att by setting on the table or floor a few saucers fille chloride of lime. If the air remain fetid, is chlorin deemed inert and inefficacious? No, surely; for the op was unskilfully performed. Thus also a small portion rine, liberated on the floor of an apartment containir and furniture, may never rise in adequate force to the the walls where contagious virus may lurk. This re peculiarly applicable to the less fugacious infections variolous, which require an energetic dose of chlori fine, one rule may serve for the bleacher and disinf this element; that is, to employ it in doses proportion stubbornness of the colouring or morbific matter.

* Mr. Faraday's elaborate fumigations of the Milbank Penitentiary under this censure.

The distribution of dilute chlorine through the cargo of a ship, and the due impregnation of the interior of the bales, may be easily accomplished by the above-described apparatus.

From the pipes G G, tubes of lead, leather, or the water-proof double cloth, are to be led down a few feet into the hold, under the main hatchway, so that by the action of the fanner the mingled airs may be driven through every interstice, till they envelope every package. The quantity of chlorine, and the continuance of the operation, must be regulated by the capacity of the ship, and the nature of the bales; but in general a couple of hours will suffice. All the openings in the deck should be carefully closed, except a small one near the stem and another near the stern, to permit the discharge of the atmospheric air and the ready circulation of the disinfecting gas. Eventually, traces of chlorine issuing from these openings will be observed by the smell or by the white cloud surrounding a feather moistened with water of ammonia (spirits of hartshorn). The process may now be regarded as complete; and after the interval of a few hours, all the hatchways and windows may be thrown open, and a free ventilation given to the ship. The residuary chlorine in its discharge into the air being wafted round the bodies of the sailors will disinfect their dress, and give final security against the importation of contagious fomes.

An apartment may be conveniently disinfected by placing on a shelf or support near the ceiling a small basin or pipkin, containing chloride of lime, having set over it a glass or earthenware funnel with muriatic acid diluted with about its weight of water; the beak of the funnel being partially closed with a cork, so that the acid may drop slowly down on the chloride. Eight ounces of good chloride thus treated with ten ounces of muriatic acid, will suffice to fumigate and sweeten the air of a common-sized chamber.

After the preceding observations, it will not be expected that I should ransack medical repositories, in proof of the anti-loimic powers of chlorine. But less fallacious evidence may be found. In the neighbourhood of the city of Glasgow, there are several large factories, the atmosphere of certain parts of which has been for a long series of years more or less

impregnated with chlorine; I allude particularly to t
mical works of Messrs. Tennant, at St. Rollox, to t
Messrs. White, at Shawfield, and to Messieurs Mc
calico-print field, at Barrowfield. In the last-mention
blishment, a great many tons of chloride of lime v
many years treated every week with sulphuric acid, i
to obtain a strong aqueous solution of chlorine. W
sulphuric acid was poured into the clear watery solutio
chloride, contained in a large leaden cistern, a very c
able quantity of chlorine gas escaped into the air, whic
municated its peculiar odour to the whole vicinity. C
was also continually emitted from the above discharged
in the course of its application to Turkey-red cloth, :
ducing the white figures of Bandana calicoes. Mr.
Rogers, the very intelligent conductor of this magnific
blishment, has just favoured me with the following let
tive to the anti-contagious influence of chlorine.

'My dear Sir,—In answer to yours of the 24th, I h
been convinced of the efficacy of chlorine in purifyi
taminated or foul air, and in arresting the progress o
gious diseases, more particularly typhus. During t
period of thirty years that I have conducted this establi
with a population of two or three thousand, includi
families, I am not aware of a single case that could be
as contagious; and in many hundred cases in which
recommended chlorine in the village (Barrowfield),
effects have been apparent in arresting the progress o
and other fevers.—'I am, my dear Sir,

'With much respect, yours,

(Signed) 'George Ro

'*Dunoon*, 31*st June*, 1831.'

Mr. White, who has given up making chloride of li
good many years, and who has no interest in the sal
article, writes me, 26th June, 1831, as follows:—

'All that I can state is merely the impression an
workmen, of their total immunity from fever;—and
pression is justified by the circumstance, that while ty

prevalent in the neighbourhood, none of the workmen employed in the manufacture of chloride of lime were ever its victims.'

(Signed) 'John White.'

As Messrs. Tennant, the original patentees of chloride of lime, are also the greatest manufacturers of it in the world, their testimony might be received as that of interested persons. But the following document from Dr. Corkindale, physician to the gaol of Glasgow, and celebrated for his skill in medical jurisprudence, is above any such suspicion.

'*Glasgow*, 1*st July*, 1831.

'In the year 1824, a suit was brought against Mr. Tennant's chemical works at St. Rollox, on the score of nuisance, proceeding on the allegation that the fumes arising from the processes there carried on were injurious to the health of the neighbourhood. Around the works there are houses for the accommodation of about twelve families of the workmen. These persons have continued to reside there for various periods from two to twenty years. I examined the condition of these people, and made inquiries as to the history of their health during their residence, as detailed by themselves. I found that their condition, in this respect, was nearly the same as other persons of the same rank of life, in ordinary situations; but it was the uniform statement of the whole of them, that *no person residing on these premises had been affected with typhus at the different periods when that epidemic was very prevalent in Glasgow.* It was evident from inspection that this immunity was not owing to superior cleanliness and ventilation, for the apartments were as dirty and crowded as the ordinary habitations, where I know typhus had prevailed. The vapours from the works were various, but by far the most prominent was chlorine, rising both from the preparation of the chloride of lime, and from the treatment of the residuum for the manufacture of soda.

(Signed) 'James Corkindale, M.D., LL.D.'

I have been favoured by M. d'Epinay, agent of the island of Mauritius to the British government, with an excellent

account of the introduction into that island of the (cholera, which, having recently transmigrated the mi Asia and the north of Europe, now desolates the provinces of Russia, and hovers like an incubus o shores. The facts it relates will be found interesti instructive in no ordinary degree.

'*London, June* 25,

'My dear Sir,—I proceed to perform the promise I made of furnishing you with some details concerning th duction of the cholera morbus into the Island of Ma This disease was imported there by the British frigate, the commanded by Captain Dumby. It is in its nature er contagious; and although this opinion exposed the co which I belong to the most violent calumnies on the pa Governor, General Darling, and to the anger of Lord B then Secretary of State for the Colonies, I must p maintaining it, because it is proved by the facts abou related.

'The Topaz arrived at Mauritius the 28th of De 1819, having just come from Ceylon, where the chol vailed. This fact is notorious, and is indeed fully ve the following extract from the *Asiatic Mirror*, publ Calcutta, the 24th of December, 1819.

'"We announce with regret that the news broug Ceylon are very distressing. Fevers, dysentery, cholera morbus are spreading in an alarming mann 7th regiment, and a detachment of the 45th, which h in the island only for a week, have suffered consi thirteen officers of the former, and thirty soldiers of t having fallen victims to this terrible scourge."

'The report of the physicians who visited the frig arrival stated, "that the dysentery and the cholera prevailed on board of it."

'Notwithstanding this, the Physician-General of tl in Mauritius and the Governor had the culpable we permit communication between the frigate and the sho rumour being universally spread that several men of tl were ill of the cholera; the representation of the C

a regiment, who opposed the patients of the vessel being carried to the Military Hospital; tents mounted to receive them in the *Ile aux Tonneliers;* were circumstances which caused uneasiness; and the members of the *commune* (parish) imparted their feelings to the Governor, who gave for answer, "that he was very sorry for them, but that he was ignorant of the laws of the colony;" an ignorance supposable enough in a military man, but not the less reprehensible in a Governor.

'On the 5th of November, he wrote to the *commune*, to inform them that the *Journal* of the following day would contain an opinion from the Physician-General, which would render useless every other measure relative to the disease which *had prevailed* on board of the Topaz.

'On the 19th of November, two negroes fell down in the street, and died before there was time to assist them; and the disease began to spread through the town. On the 23rd, the frigate brought-to, and visited a boat from the shore, as it came out of the harbour, on its way to the river *Rempart.* The crew of this boat were soon thereafter attacked with the cholera, which they communicated to the establishment of M. Carcenau, their master, who lost forty slaves, and died himself of the same disease. This was the first plantation where the cholera showed itself, although it was six leagues from the town. It soon made the tour of the island, terrible in its first ravages, but becoming milder by degrees; more fatal in the neighbourhood of the sea, and unknown in elevated regions.

'The communications with the island of Bourbon, thirty leagues from Mauritius, being open, the disease was not long in being carried thither. The inhabitants, taking alarm, formed immediately a *cordon* round the town of St. Denys, and the punishment of death was decreed against all who should dare to break through it. This scourge did not extend beyond the limits which wise and courageous men had here traced around it.

'Very different was the case at Madagascar, into which the cholera was imported from Mauritius, and exercised the greatest ravages.

'It was computed that, in our island, the number of its victims amounted to a tenth of the population; and I concur

in this estimate. It was chiefly among the lower classes, persons given to intemperance, that the cholera was fatal.

'A healthy regimen, great cleanliness, exercise, recrea and courage, were found to be the best preservatives ag its attack. Individuals who wore flannel generally esc The most successful remedy was saline purgatives, rep in half-ounce doses every quarter of an hour, till the discharge assumed a natural colour.

'I could enter into other details which go to fortif opinion I have always entertained of the contagious nat cholera; but I think those I have adduced, and partic what happened at Bourbon, ought to convince you th many precautions cannot be taken against permitting co nications with vessels coming from districts infected wi malady. Believe me to be, my dear Sir,

'Your devoted Servant,

(Signed) 'AND. D'E

From the same. '*July* 7, 1

'I told you that, in the Isle of France, dur cholera, we employed as a disinfector a mixture of o manganese and muriatic acid. We provided small pl it, which were carried about in all the infirmaries, and people who entered the hospitals. They were also about by the women and children; and it was remar none of those so protected by the disinfecting phia attacked with the disease. Was this from the virtue composition, or from the confidence inspired by it? answer these questions, but content myself with sta fact.

(Signed) 'AND. D'

*** The cask A (in both cuts) should have a plug-hole or stop-co bottom (not shown in the figure), for discharging the liquid muriate have omitted to state that dyed silk may be treated without injur chlorine.

ON THE PENETRATIVENESS OF FLUIDS*.

By J. K. MITCHELL, M.D., Lecturer on Medical Chemistry in the Philadelphia Medical Institute.

[The generality and importance of this paper is such that we think it quite impossible to convey any idea of it by an abstract, and feel ourselves bound to bring it before our English readers at full length.]

IN 1829, I read before the Philosophical Society, a short memoir on a new method of forming gum elastic into thin plates, sheets, and bags. In some instances the balloons formed by the process then described had, when filled with hydrogen gas, the power of ascending to a considerable height in the atmosphere. Those which were confined to the atmosphere of my lecture-room, at the Medical Institute, descended again after a period of time, varying from an hour to two days. The cause of the descent, which did not seem of easy explanation, became a subject of investigation.

The gas might have escaped from the balloons at the ligature, or by permeating the dense wall of gum elastic, or by uniting chemically with the internal surface of the latter. To free the gas from the compression to which it is subjected in a balloon, I confined it in a wide-mouthed bottle, over the aperture of which I tied very firmly a thin sheet of the elastic membrane. In a few hours the descent of the cover into the cavity of the bottle gave evidence of a diminution of the contained gas, and finally the cover was burst inwards by the pressure of the atmosphere, so great had been the rarefaction of that which remained in the bottle. On weighing the membranous cover, no gain in weight could be perceived, so that I presumed that the gas had escaped. By repeating the experiment, and covering the bottle with a small bell-glass holding atmospheric air, I found, after a time, in the latter vessel, an explosive mixture, while the contents of the bottle itself were found to be pure or nearly pure hydrogen. Evidence was thus afforded that hydrogen penetrated the membrane not by any *vis a tergo*, for no pressure was applied, but by some inherent power of considerable amount. The facility of permeation appeared also much greater in the hydrogen than in the atmospheric air, which, if it entered at all into the bottle, did not penetrate in any appreciable quantity, when fully one-half of the hydrogen had made its escape.

In the next experiment the arrangement of the gases was altered: common air was inclosed in the bottle, and a bell-glass confined around it in an atmosphere of hydrogen. As was expected, the

* Philadelphia Journal of Medical Sciences, xiii. 36.

hydrogen entered the bottle rapidly, raised up the tense meml formed it into a globe, and finally burst through it, and thus its escape from the confinement to which it had been spontan subjected.

The minuteness of the atom of hydrogen might readily e account for the greater facility with which it penetrated the brane, but could not be considered a good reason for the with which the penetration was accomplished. A gas having a atom was therefore selected for further experiment, and ca acid, subjected to the same sort of confinement, was fou permeate the membrane with as great power, and very greater facility. In succession, most of the gases were subm the same ordeal, and all of them found, except nitrogen gas, ercise the same power, but with very different degrees of r The *power* was ascertained by comparison with common air, *rate of action* both in that mode and by comparison with eac The depression or elevation of the membranous cover clea cated the escape or entrance of a gas, and when two activ were placed one on each side of it, its rise or fall expressed ference of rate, because each was, at the same moment, in th permeation, as proved by many examinations of the content bottle and bell-glass.

Having once ascertained the rate of action of each gas re air, a prediction could be made as to their rate in reference other. Hence gases which operated on air with nearly eq city, affected the *horizontality* of the membrane very lit placed on opposite sides of it. Thus carbonic acid and nitr act with great facility on common air, and in nearly equal and when placed on opposite sides of the membrane, pe rapidly, but cause a very slow change in its position. here presented warrant the conclusion, that if two gases penetrant *exactly*, could be found, they would, under t described arrangement, mix uniformly, without in the degree altering the state of the membrane *.

The greatest possible degree of effect on the membra when we place on opposite sides of it the slowest and m penetrator; for instance, nitrogen and sulphuretted hydr that case the change is immediately visible.

* Subsequently having discovered that olefiant gas and arsenurette have, with reference to common air, exactly equal rates, they wer opposite sides of a membrane, with a full expectation of sustained on the part of the membrane; which was confirmed by the result.

As in all the previous experiments, different gases were placed in comparison, I placed the same gas on both sides, and expected, for the 'sufficient reason,' no change. The experiment accorded with expectation. The membrane remained stationary.

The circumstances *essential* to the transmission of gases through the membrane formed an interesting subject of inquiry.

My first attempt was to produce a vacuum, by placing the gas in a bottle, and exhausting, by means of the air-pump, the bell-glass which covered it. The gases effected their escape from the bottles thus treated, with a velocity proportional to the rate of permeation already ascertained; sulphuretted hydrogen passed out more rapidly than carbonic acid, and that than hydrogen. Still as *some* air is always found in an exhausted receiver on the finest air-pump, I passed a tube containing carbonic acid into a Torricellian vacuum, where it very speedily escaped and caused the descent of the mercury. Even this experiment could not prove perfectly satisfactory, as mercurial vapour occupies the barometric vacuum. A perfectly empty bag carefully closed was placed in carbonic acid and nitrous oxide successively, without undergoing the slightest inflation. If a very small portion of *any kind* of air remained in the bag, inflation followed, provided the bag were exposed to a *different* gas.

By another arrangement I obtained my object more unexceptionably. Having found, by inverting a bottle holding confined gas, and thus plunging it into mercury, that no gas escaped, and that consequently mercury could not promote or sustain the permeation of the gas, I reached my object by the following means. Closing a tall cylindrical lamp-glass at *one* end with gum elastic, and filling it with mercury, it was placed, so filled, on the shelf of the mercurial trough, having the end closed by the membrane uppermost. Through this fine film the mercury could be plainly seen in close contact with its under surface, while the deep depression of the membrane showed the power of the column of mercury by which it was drawn down. By leaving it in the air, or by placing over it a bell-glass of any gas, more slowly, but at their settled rates, the gases penetrated the membrane and accumulated in the cylinder, thus permitting the descent of the mercury. The process continued long after the mercury had abandoned the surface of the membrane, and the space was occupied by the gas, in, of course, a rarefied state.

It became then evident, that anything which could remove the gas from the surface of the membrane at which it had arrived by penetration, would continue its transmission. Of course then

agents chemically attractive of a particular gas, when placec neath the membrane, would promote its permeation. In fact, water and solution of baryta were rapidly carbonated by the t mission of carbonic acid, and sulphuretted hydrogen almost inst precipitated the lead of the acetate placed in solution on the op side of the membrane, which became black on the side of the tion. A neater mode of performing this experiment is the follo Inject by means of a gum elastic bottle and pipe, into a very bag of gum elastic, stretched until fully transparent, a solut the substance to be acted on. Carefully tied, washed, and drie bag is to be passed up through mercury into a receiver holdi gas, which for solution of baryta should be carbonic acid, a that of acetate of lead, hydrosulphuric acid. In a few mome the former case, a white coat is seen to completely line the i surface of the bag, and in a few minutes to fall down and ac late at the bottom of it. In the latter case, the inner coat i indelibly black. In either case, if water be alone placed in th it will absorb a considerable quantity of either of these gas their presence may be ascertained by the usual tests.

If any suspicion had arisen that the gases escaped or ent the route of the space included under ligature, it was dissip all the experiments mentioned in the last section; inasmuc the first experiment, that with the lamp-glass, the gas was stud beautifully the under surface of the membrane, standin in minute drops or bubbles, mistaken at first for water. experiments with baryta and lead in bags, the whole surf covered, the precipitation taking place *only* there. Especi it manifest in the last experiment, where the inner surf stained black, while the solution remained clear and colourle gas, therefore, penetrates through *every part* of the membra

Being desirous of ascertaining more accurately the relative of transmission, I solicited the assistance of my friend an Professor J. K. Finley, to whose patience, skill, and manipulation, I owe much of the certainty of the following ments.

Having constructed a syphon of glass with one limb thr long, and the other ten or twelve inches, the open end of leg was enlarged and formed into the shape of a funnel, o finally was firmly tied a piece of thin gum elastic. By this syphon and pouring into its longer limb some clean n portion of common air was shut up in the short leg, an communication with the membrane. Over this end, in t

rial trough, was placed the vessel containing the gas to be tried, and its velocity of penetration measured by the time occupied in elevating to a given degree the mercurial column in the other limb. Having thus compared the gases with common air, and subsequently by the same instrument, and in bottles, with each other, I was able to arrange the following gases according to their relative facility of transmission, beginning with the most powerful:—ammonia, sulphuretted hydrogen, cyanogen, carbonic acid, nitrous oxide, arsenuretted hydrogen, olefiant gas, hydrogen, oxygen, carbonic oxide, and nitrogen.

Ammonia transmitted in 1 minute as much in volume as sulphuretted hydrogen in 2½ minutes—cyanogen, 3¼—carbonic acid, 5½—nitrous oxide, 6½—arsenuretted hydrogen, 27½—olefiant gas, 28—hydrogen 37½—oxygen, 1 hour and 53 minutes—carbonic oxide, 2 hours and 40 minutes.

Nitrogen has a rate of penetration so low as to be difficult to ascertain, because there is no gas of a lower rate with which to compare it. Only by causing it to pass through a membrane by means of a column of mercury, is the fact of its transmission known. In that way, the quantity being compared with that of carbonic acid, its rate was found to be about three hours and a quarter*. This experiment, made but once, is not confidently relied on; but the rate of nitrogen is unquestionably less than that of carbonic oxide.

Chlorine immediately altered the texture of the membrane, as did muriatic acid gas, sulphurous acid, nitric oxide, and some others, so that it was impossible to reach, for their rate of penetration, accurate results.

In every case the movement of the gas through the membrane became progressively slower, until it totally ceased; and finally, but more slowly, the mixed gas returned, as indicated by the descent of the column of mercury. The retrogradation ceased only when the two columns came to equilibrium, or, failing the possibility of that, when the mercury in the shorter limb had reached the membrane, through which mercury has not been found able to penetrate.

Acquainted with the *fact*, and the relative *rate* of the penetrativeness of gases, the *degree of force* became the next subject of inquiry:

* A vessel filled with atmospheric air and closed by gum elastic was submerged under water for two weeks, when it was found to contain only nitrogen gas. Possibly this arrangement may furnish a new eudiometer. It offers a new mode of obtaining nitrogen gas.

A phial containing atmospheric air, after being closed by a membrane, was placed in a receiver holding nitrous oxide. In about two weeks only nitrogen was found in the phial. These facts show the mechanically sluggish character of nitrogen *gas:* with its chemical inactivity we have been long acquainted.

that it was considerable could be seen by looking at the stout
branes broken by it.

By greatly increasing the length of the taller limb of an in
syphon, similar to the one already described, I was able to bri
bear on the common air imprisoned in the shorter limb, a very
siderable column of mercury. Up to a pressure of sixty-three i
of mercury or rather more, equal to more than the power c
atmospheres, the penetrative action was found capable of con
the gases, the subject of the experiment, into the short leg, th
the gum elastic membrane. The entrance of the gas into the
leg was expressed by the ascent of the long column of merc
the other, which as it entered, it was compelled to heave up
the height of sixty-three inches, the membrane, though sup
by cloth, could scarcely sustain the weight, and would not be
increase of height. Although, therefore, at present, I do not
the limit of this power, I believe it will be found very much g
because the power of the column which was tried did not,
leak was sprung, seem to very sensibly affect the rate of entr

To the mind of a physician, the repetition of the foregoing
ments, substituting animal membranes for gum elastic, woul
rally suggest itself. Should animal membranes present the
phenomena, the interest of the investigation would be vast
hanced, and a very important service done to the cause of '
ological Medicine.' That animal membranes would act in th
manner was rendered probable by the well-known experim
PRIESTLEY, who affected by means of oxygen the colour of
confined in a bladder. It had also been observed by him
closely tied bladder, containing hydrogen gas, is found,
considerable lapse of time, to contain only atmospheric air, a
in quantity perhaps equal to the hydrogen lost. Several oth
of the same kind are detailed by him. Finally in the *Journa*
Royal Institution, I find the following 'Notice of the S
Inflation of a Bladder. By THOMAS GRAHAM, A.M.,F.R.S.E
turer on Chemistry, Glasgow.'

'In the course of an investigation of mixed gases throug
lary openings, the following singular observation was made.

'A sound bladder with stop-cock was filled about two thir
coal gas, and the stop-cock shut; the bladder was passed up
flaccid state, into a bell-jar receiver, filled with carbonic a
over water. The bladder was thus introduced into an atm
of carbonic acid gas. In the course of twelve hours, ins
being in the flaccid state in which it was left, the bladder wa
distended to the utmost, and on the very point of burstin
most of the carbonic acid gas in the receiver had disappeare

bladder actually burst in the neck, in withdrawing it from under the receiver. It was found to contain thirty-five parts carbonic acid gas by volume in one hundred. The substance of the bladder was quite fresh to the smell, and appeared to have undergone no change. The carbonic acid gas remaining without in the bell-jar had acquired a very little coal gas.

'The conclusion is unavoidable, that the close bladder was inflated by the insinuation of carbonic acid gas from without.

'In a second experiment, a bladder containing rather less coal gas, and similarly placed in an atmosphere of carbonic acid gas, being fully inflated in fifteen hours, was found to have acquired forty parts in one hundred of this latter gas, a small portion of coal gas left the bladder as before.

'A close bladder, half filled with common air, was fully inflated in like manner, in the course of twenty-four hours. The entrance of carbonic acid gas into the bladder depends, therefore, upon no peculiar property of coal gas. The bladder partially filled with coal gas did not expand at all in the same jar containing common air or water only.

'M. Dutrochet will probably view, in these experiments, the discovery of *endosmose* acting upon aëriform matter, as he observed it to act upon bodies in the liquid state. Unaware of the speculations of that philosopher, at the time the experiments were made, I fabricated the following theory to account for them, to which I am still disposed to adhere, although it does not involve the new power.

'The jar of carbonic acid gas standing over water, the bladder was moist, and we know it to be porous. Between the air in the bladder and the carbonic acid gas without, there existed CAPILLARY CANALS through the substance of the bladder filled with water. The surface of water at the outer extremity of these canals being exposed to carbonic acid gas, a gas soluble in water would necessarily absorb it. But the gas in solution, when, permeating through a canal, it arrived at the surface of the inner extremity, would *rise* as necessarily into the air in the bladder and expand it. Nothing but the presence of carbonic acid gas within could prevent the disengagement of that gas. The force by which water is held in minute *capillary tubes* might retain that liquid in the pores of the bladder, and enable it to act in the transit of the gas even after the pressure within the bladder had become considerable.'

A careful perusal of Mr. Graham's notice will excite in every one who knows the value of experimental interrogation, an expression of surprise, at the failure, on the part of that intelligent and ingenious chemist, to pursue, in the only true spirit of science, the investigation of a principle, one of the most striking manifestations of which had thus been placed conspicuously before him. Content with a single additional experiment, he comes, *in the ancient method*, to immediate conjectural explanation, and has thus lost an easy opportunity of making a beautiful, and, perhaps, extensively useful

discovery. Made at an earlier period, his observation was publ in the Journal for October, 1829, and has since attracted rently no scientific attention. Such is usually the fate of the pregnant facts which are not perceived to bear on some *gene1* This one passed from *my* mind along with all the other isolatec nomena of that number of the Journal, and only shone impo1 when illuminated by the reflected light of an extensive pri1 subsequently developed. These remarks are made, not to thro discredit on the character of the accomplished gentleman to they refer, but to correct the baneful error of ancient dogm which yet weighs so heavily on the cause of nature and trutl was true that the carbonic acid entered a closed bladder, an too *with power*, and it was equally true, that oxygen had the same thing in the experiment of Priestley, and that, hands, even common air had penetrated to replace hydro a similar viscus, and yet he ascribed the phenomenon ob by him to the *capillaries*, and the conducting power of *a canals*.

In what manner the *power* of 'rising into the air' was giv whether it was dependent on the force of water, or some othe does not and could not be made to appear from the single presented by Mr. Graham. A very little practical interr following the *word* just uttered by nature, would have obta answer fraught with new and important truth.

But to return to the immediate subject of this essay.—A the experiments of M. Dutrochet, and the observations of I and Graham, gave me almost the certainty of finding anima branes performing relatively to the gases the same functio belongs to those formed of the inspissated juice of the *Iatro tica*. Accordingly, each gas was subjected to the action o membranes, which replaced the gum elastic at the mout short limb of an inverted syphon. Dried bladder, and gold skin, moistened to cause an approach to a normal state, and of various recent tissures, were successively tried, and fou on the gases in the manner and order in which they were by gum elastic. The more *recent* the membrane, the m and extensive the effect produced; and in *living animals* t mission was very rapid.

Besides the estimates of comparative movement made syphon, experiments in a different manner were resorted to clearly show the general truth. Thus a piece of the strong of a goose connected with the œsophagus and gizzard, be tially inflated with common air, and firmly tied, was 1

atmosphere of carbonic acid, where *in less than ten minutes* the inflation caused it to burst. On repetition of this experiment and examination before fracture, a very large quantity of carbonic acid gas was discovered to have entered the intestine. Crop, bladder, &c. &c. of recently killed animals produced exactly similar results. Perhaps the following experiment will be esteemed even more satisfactory. Carefully removed from the chest of a snapper, (*Testudo serpentaria*,) its lung was partially inflated with common air, and confined there by a ligature on the tracheal tube. Exposed in this state to an atmosphere of carbonic acid, or nitrous oxide, it became very soon fully inflated by the gas to which exposed, as subsequently proved by chemical examination. Less than half an hour of exposure sufficed for the full inflation of the lung, which was removed only when it threatened to burst. Containing a portion of nitrogen, it was left exposed all night to an atmosphere of oxygen, yet scarcely enough entered to signify its presence; in quantity superior to that which is held in atmospheric air. A taper appeared in it somewhat brighter than before its immersion.

In a subsequent experiment, the two lungs of a snapper having been extracted, were inflated respectively, with common air, and carbonic acid gas. So prepared, each lung was surrounded by a bell-glass containing an atmosphere of the other gas, so that common air surrounded the carbonic acid, *et vice versâ*. That lung which contained common air soon burst by the infiltration of carbonic acid, while the other collapsed by its escape.

In concluding the series of experiments on *the question of fact*, some were made on *living animals*. A quantity of solution of acetate of lead having been thrown into the peritoneal cavity of a young cat, sulphuretted hydrogen was discharged from the pipe of the generating retort, directly into the rectum. In four minutes the poisonous gas killed the animal, giving to it, because of enormously dilated pupils, a very wild aspect. Instantly on its death, which was itself an affair of a moment, the peritoneal coat of the intestines, and the walls of the cavity in contact with them, were found lined with a metallic-looking precipitate, adherent to the surface, and susceptible of removal by nitric acid, moderately diluted. It was the characteristic precipitate of sulphuretted hydrogen when acting on lead. When, in another experiment, the abdominal cavity was almost instantly opened, only the intestines and stomach presented the bronzed aspect; the peritoneum of other parts, and the bladder, appeared of their natural colour, thus proving that the gas had infiltrated, and not passed through any rent or fracture, an event

which would have stained the whole of the lining membran cavity, and dyed the bladder. This experiment forcibly re us of that where the internal surface of a gum elastic bag lead water, was stained black by sulphuretted hydrogen, w solution continued pellucid.

In another experiment on a cat, a solution of acetate of l placed in the thorax, and sulphuretted hydrogen in the ab Almost immediately, on the entrance of the sulphuretted hy into the abdominal cavity, death ensued, with the same di of pupil as before. On inspecting the thoracic side of t phragm, which was done as quickly as possible, the tendin of it displayed the leaden aspect of the precipitate by sulph hydrogen. Many years ago, in 1823, while engaged in gating MAGENDIE's theory of venous absorption, I colou diaphragm of a living cat blue, by placing a solution of prus potash on one side, and that of sulphate of iron on the oth that time I supposed the effect to be *vascular*, but the expe on membranes of gum elastic afford an explanation whi rationally refers it to *organic molecular infiltration;* for, membranes, vessels cannot possibly exist at all; and as membranes act in a manner so perfectly accordant with tha coagulated vegetable juice, it would be judging against e to refer their agency to widely different causes. At the san tive rates, with the same power, and that a great one, the scarcely act, in obedience to causes so dissimilar as those allı

Every one who has read the beautiful memoir of Dutro '*L'agent immédiat du Mouvement Vital, &c.*,' and who nearly all have, suffered their belief to be swayed by his el of fact, method, and style, will, on a cursory glance at the ments detailed in *this* paper, refer them to the 'NEW PO ably contended for by the French naturalist. That they de the *same power* cannot be reasonably questioned, whether tha be one long known or recently discovered. In his experimen exclusively on liquids, and developed with surpassing good and sagacity, he proved the transmission of liquids through membranes, and saw them penetrating, too, *at different rat* solutions passing rapidly, some with greater slowness, s scarcely appreciable quantity, and some never passing at all *force*, too, he found to be of estimable amount. In fac aspect of the two sets of experiments tends, more and more to induce a reference of them to one and the same cause, w that cause may be. Although the facts presented by him

strate all this, yet M. Dutrochet did not perceive it, as is evident from his reference of the phenomena to a source to which, in latter years, the French naturalists and philosophers have been accustomed to look with almost superstitious reverence. Electricity is the great key of scientific explanation; and the theory of Du Fay is relied on, though badly itself sustained, as the *point d'appui* of almost all other theories. M. Dutrochet has accordingly ascribed the transmissions to that power, and supposed, in the very teeth of some of his most striking facts, that the current was from a less dense to a more dense fluid, or from positive to negative, dependent not on an inherent power of infiltration, and of course for the same membrane always the same, but varied, or even inverted, at pleasure, by arrangements productive of supposed electrical powers. He says, p. 139,

'Ces résultats nous font déjà pressentir que l'impulsion qu'éprouvent les liquides dans ses expériences, dépend d'un courant électrique déterminé par le voisinage de deux fluides de densité ou de nature chimique différentes, fluides que sépare imparfaitement une membrane perméable. *Cette membrane ne joue évidemment aucun rôle propre dans cette circonstance; elle ne fait fonction que de moyen de séparation entre les deux fluides* auxquels elle est cependant perméable: *les liquides la traversent, soit dans un sens, soit dans l'autre, au gré de l'action réciproque des deux fluides qui baignent ses parois opposées.*'

As he used water and solutions in water, by which the former became denser, he found, as might be expected, that it infiltrated the tissue more readily than most of its solutions; hence, in such cases, the water penetrated more quickly than they, and the current usually set most rapidly from less dense to more dense. But when he used essentially different liquids, he yet found the water going through at *its high rate,* as we perceived to be the case with sulphuretted hydrogen and ammonia. Water traversed the animal membrane rapidly to join *alcohol,* which, according to his electrical theory, should not have been the case, as the alcohol is less dense than water. For this and some other exceptions Dutrochet attempts to account, by reference to influence derived from *chemical qualities.*

If, however, as in the case of the gases, two *liquids* of different rates of penetrativeness be placed on opposite sides of an animal membrane, they will in time present the greater accumulation on the side of the less penetrant liquid, *whether more or less dense,* but will finally thoroughly and uniformly mix on both sides, and at

length, if any pressure exist on either side, yield to that and the other side.

As some substances have no penetrativeness, such as blood, or at least their solid parts, the water placed on the side of the membrane alone moves, and it is only after the position by putrefaction, and consequent formation of a *n having penetrant properties*, that any current sets in the opposite to that of the water. To prove this, it is only nece show that *alcohol* penetrates gum elastic much more rapi *water;* and that, therefore, when that kind of membrane i posed between them, the greater current is from alcohol tc and not from water to alcohol.

A hollow glass cylinder, open at both ends, was clos pletely by two membranes of gum elastic, having been pr perfectly filled with alcohol. It was then sunk in the larg matic trough of my laboratory, where it remained one we that time it presented a concavity at each end, of decidec proving the escape of a considerable quantity of alcohol. other hand, a similarly prepared vessel filled with water a merged in alcohol presented at the end of a week well *convexities*, demonstrating the insinuation of alcohol. If it tended that the nature of the membrane affects and even the electrical state, it may be well said in reply, that ther analogy for that, and moreover, the same membrane acts ui movement of *gases* precisely as an animal membrane. The sition would invest it with a most Protean character.

In making experiments for the preparation of gum el ether, that liquid was found to readily infiltrate its tissue. has been already shown to penetrate it better than water, an enters its substance so slowly, that a bag of a thinness prod almost perfect transparency, and containing four ounces, tw and fifty-seven grains, lost by evaporation but eight grain first period of twenty-four hours, and fifteen grains during three days. Viewing these facts, a prediction was founded relative to the effect of placing ether in contact with one su such a membrane, while alcohol or water occupied the oppo face. As was expected, the greater quantity accumulated side of the less penetrative substance, and the ether always by its transmission, an augmentation of liquid on the sid alcohol or water. Using *animal membranes*, facts of a simi previously ascertained, led us to anticipate the *opposite* resu cording to expectation, water being most penetrative, passed

so much more rapidly than ether or alcohol as to swell the amount of liquid on their side.

When alcohol is largely diluted with water it penetrates an animal membrane more easily itself, and offers to the pure water which reaches it from the opposite side less invitation to infiltrate it, according to a law of *progressive diminution,* pointed out by our experiments on gases. Such a diluted portion of alcohol placed by M. Dutrochet in his endosmometer, and raised above the level of the pure water on its outside, found, in the force of the higher column, sufficient cause for its escape, which continued until the level was reached, when action apparently ceased. If the level be obtained at the commencement of the experiment, either no appreciable change is observed, or the movement is unquestionably in a direction contrary to that stated by Dutrochet. So, when gases are permeating in opposite directions any interposed membrane, the penetration soon begins to lessen, because there is on either side less porosity unoccupied, and there is also in them the repellent character of their gaseous state. M. Dutrochet reconciles these apparently contradictory facts to his system, by supposing chemical influence to produce the first, and electricity the second. In either case, he does not appear to dream of independent and original powers of penetration, by which the liquid comes through to the opposite side of the membrane, *remaining* in *its* tissue, or passing on by a similar power of infiltration into new matter, or, such matter being absent, accumulating on that side by the influence of mechanical power, or electrical excitement, or chemical combination, truths adequately demonstrated by my experiments on gases.

The blinding effect of preconception on the most philosophic and candid mind can perhaps have no better exemplification than is afforded by what M. Dutrochet says relative to the point of accumulation, when a *diluted acid* and water were placed on opposite sides of an animal membrane. As alkalies produced towards them a current for the support of his electrical theory, acids should be found to set the current towards water, and *he found it so.* In my experiments, the greater current was *always* towards the acid, and not from it; and I find that Dr. WEDEMEYER (Untersuchungen über der Kreislauf des Bluts, &c.) has made the experiment with a like result. On reference to Dr. TOGNO's experiments (*Amer. Journ. of Med. Sci.*), which were chiefly repetitions of those of Dutrochet, we perceive that he does not seem to be satisfied perfectly with the report of Dutrochet on *this* subject. Let any one desirous of testing this matter, tie a piece of animal membrane over the end of a hollow

glass cylinder, partially fill it with diluted sulphuric acid it in a vessel of clean water, so as to bring the two co level. In a few hours the column holding the acid wil siderably above that of the clean water, proving the grea to set from water to acid, and not from acid to water. T ever, show that *some* acid does pass the membrane *.

To feel *assured* of the error of Dutrochet, I repeated ment in another form. A tube of five-sixteenths of an i meter, ending in a funnel-like extremity of an inch and was covered at its broad end by animal membrane, the filled with diluted acid, and placed, membrane downward water, so as to bring both columns to a level. INSTANT in the narrow tube was perceptible, and amounted to nea inch in half an hour. Reversing the order, by placing water in the tube, and the diluted acid without, as sudde gressive a descent of the column of clean water was Tests, after a short time, betrayed the percolation of som finally, in every case the liquid became uniformly acidulou out, and the two columns fell to a common level—an e may always be expected, unless the combination produce mission is not penetrant.

Water may be removed from the surface of a membran it has arrived in many and various methods. Invitati given to it by a column of mercury contained in a hc der closed above by animal membrane. Water read through, may be seen studding in drops the surface of th gradually covering the under side of the membrane, length the separation and descent of the mercury, and co enter the cylinder until the mercurial column sinks to t the general contents of the trough. There the action ce the water placed *above* the membrane be now removed, rial column will again rise, and all the water having escap the membrane by the process of infiltration into the atmo mercury will be finally seen in close contact with the from which it had receded. Sometimes before the co the process a change takes place in the condition of t matter, and some gas being introduced below suspends of the mercury †.

* This fact I demonstrated to Dr. Togno.

† A new hygrometer was suggested by this experiment, of whic giving an account to the Philosophical Society.

A sponge *slightly* moistened, or dry oat-meal, or any other absorbent, placed by means of a moderate weight closely in contact with the membrane, will, by absorbing the water, cause its continued permeation.

Even *vis a tergo*, as in the instance of the gases, will produce infiltration where there exists no other cause of penetration. Over the end of the short limb of an inverted syphon was tied a piece of bladder, and over that, and *in close contact with it*, was also secured a piece of sheet caoutchouc. Water was then placed in the short limb in communication with the bladder, and thus left for a few hours without compression. No appreciable amount of infiltration ensued. But, in a short time after a column of mercury had been placed in the long limb, water was plainly seen to insinuate itself through the bladder, and to raise up and separate from it the more elastic membrane which surmounted it. After all the water had passed into the space between the two membranes, the syphon was placed in its ordinary position, the end of the long limb resting in the mercury of the trough. Soon the water repassed the bladder, ascended through the short column of mercury lying above it, and collected in the curve which then formed the pinnacle of the apparatus.

Another fact, in itself important, bears forcibly against the electrical theory of Dutrochet. To try the absorbent power of the dermoid tissue, pieces of it in a recent state were tied, cuticle outwards, over bottles which contained common air, or carbonic acid gas. Over the bottle which held carbonic acid was inverted a jar of common air, and over that holding air was placed a jar of carbonic acid. The more penetrating gas was, in the first case, in contact with the cuticle, and in the other, with the dissected under surface of the skin. A trial of the contents, after twenty-four hours, showed that much more carbonic acid had penetrated in that apparatus where it was applied to the cuticle, than in the other. As in that case it had gone from the jar into the bottle of common air, while in the other case very little carbonic acid gas had escaped from its receptacle, I filled it again, and tied over it a piece of skin with its cuticle looking inwards. In twenty-four hours the carbonic acid was equally diffused through both bottle and jar. Two similar sections of intestine were slightly inflated with common air, one of them being turned inside out. Both having been carefully tied at the ends, were placed in identically the same carbonic acid in vessels of equal size. It was soon apparent that the one which had been inverted, filled itself

most rapidly, and although rather less than the other, soon gr
exceeded it in size and hardness. After remaining so expose
eighteen hours, vessels of common air were placed over th
tended bags, when a diminution of volume became in time appa
and was more rapid considerably in the specimen which ha
been inverted. It appears, then, that the transmission of a g
easiest where it is placed on the cuticular or mucous surface
animal membrane, rather than on its cellular or peritoneal su
—a fact to be kept in view in rating the transmissibility of th
ferent gases or liquids. The fluids should be compared unde
actly similar circumstances, standing in the same relation t
surfaces of the membrane used.

In the following experiment, made with great precaution, w
ceive a result distinctly indicative of the superior penetrability
cuticular surface. Over the mouths of two phials, accurately
with alcohol, weighing, according to a *Pese-Ether*, thirty-fiv
a quarter, were tied two pieces of human skin. In one the ra
presented, in the other the cuticular side. Both were placed
downwards in similar specimens of water, with columns of
altitude. After the lapse of twenty-four hours, the alcohol wa
mined, and found to weigh more, by at least one degree,
phial which presented the cuticle to the water. In it the e
meter sunk to thirty-three and a half, while in that which pre
the dissected surface to the water it fell only to thirty-four
half. The one had been reduced by the water one degree and
fourths, and the other only three-fourths of a degree.

In all these cutaneous experiments, we perceive not or
agency of the membrane itself, but even that of its respecti
faces, so that we are not at liberty to admit the assertion res
the action of the liquids, as independent of the influence of th
vening membrane.

In truth, it is now manifest that the liquid, if penetrativ
meates a given tissue at a rate dependent on the character o
and power of penetration. If on the opposite side there exist
stance or power capable of occupying or removing it as fas
faster than the membrane delivers it, the actual rate of trans
will be as high as is possible; but if not so capable, the accum
will be at a lesser rate, and will represent the degree of perm
of the inviting substance alone. Thus, for illustration, if et
convey away water as fast as, or faster, than the membr
transmit it, the rate of penetration will be the greatest possi

will represent the full penetrability of that membrane by water. But if ether is less penetrable than that membrane, the rate of accumulation will not represent the power of the animal tissue, but that of the ethereal interstices, which, on the supposition, is less.

The *power* of this process in liquids, like that of the gases, is not *yet* measured. It is the *power of infiltration* in all such cases, and must be eminently great. Like all processes having dependence on molecular action, this one is influenced by electricity, when that is brought to bear on it, but we can scarcely, after a fair estimate of the value of facts, see anything more in the power than that of common interstitial infiltration, a power marvellously great, but insusceptible of demonstrative reference at present to any known cause.

The amount of force having been shown to greatly exceed that of atmospheric pressure, we feel assured that the interstices are penetrated not by any *vis a tergo*. It must therefore be attributed to some species of *attraction*, the force of which, as shown by the condensation of some gases by charcoal, sometimes equals a power of forty atmospheres, or nearly six hundred pounds on the square inch, a power amounting nearly to that of steam, *at its maximum density* *. It is not chemical, because the quantity absorbed bears no relation to known affinities; it is not homogeneous attraction, for it takes place *solely* among dissimilar substances, and often subverts the condition produced by that power, as in some cases of solution.

After having proceeded thus far with my argument and experiments, I felt as if it were important, if not essential, to my positions, to test the power of gum elastic as an absorber of gases, independently of the artificial arrangements which brought different gases to the opposite sides of it. For that purpose I selected a hollow cylinder of gum elastic, with thick parietes about an inch in length. This specimen was placed in a cylindrical graduated test-glass, filled with carbonic acid gas and placed over mercury. In less than one minute the mercury began to rise, and in eight hours, during which the observer was absent, it had risen to a considerable height. A rough attempt to measure the bulk of mercury raised, and of gum elastic used, showed that nearly an equal volume of

* Found by comparing the experiments of Cagnard de la Tour with those of the Committee of the Institute of France.

carbonic acid had been absorbed by the caoutchouc. A piec dry bladder was subjected to the same treatment, and produc similar rise of the column of mercury. Macerated in water fo hour, and then wiped well with a dry towel, so as to obtain dry faces, the same piece of bladder was again placed in the gas mercury, and produced a diminution apparently equal in quanti that which, when dry, it occasioned.

The bulk of the gum elastic was considerably increased b infiltration, so that, although easily placed in the glass vess was of difficult removal. This fact, added to that of the thor penetration by water of an animal membrane macerated in it, sl how much of the phenomena described in this paper is attribu to the organic molecular infiltration. The remainder of the is dependent on the moleculo-porous relation of the gas or liqu the substance beyond, into which infiltration carries the perme substance. If the recipient beyond the membrane be as acti the membrane, or more so, all that the membrane brings to it face will be transmitted as fast as it arrives; but if that recipie of inferior penetrability, less will pass on than the membrane carry through, and in that case the rate of penetrativeness o substance relative to the membrane is inappreciable. An penetrates another gas better than it does any solid, hence w tain for *them* the true rate. But liquids penetrate each other times less rapidly than at the rate of the transmission throug membrane. Such cases do not show the rate of transmissibil the membrane, but of reception beyond.

[To be continued.]

Proceedings of the Royal Institution of Great Britain.

FRIDAY EVENING MEETINGS, 1831. (CONTINUED.)

April 15*th.*—Mr. J. F. Daniell *on the Forms and Attractions of the Particles of Crystals.*—The subject of this evening forms the matter of the paper at page 30 of the present Number of this Journal.

The recent experiments made in Edinburgh by Mr. A. Trevellyan, on the production of musical sound during the transference of heat, by conduction, from hot pieces of metal to cold masses of lead, were repeated before the members, with apparatus brought from Edinburgh, by Mr. Addams.

April 22*d.*—Mr. Marshall *on the Origin and Utility of Cow-pox; with the Causes of Failure in the practice of Vaccination.*—Mr. Marshall introduced the subject by a short account of Dr. Jenner and his exertions. He then proceeded to notice the effects of the practice of vaccination, and the causes of its occasional failure. From tables it appeared that the annual mortality in cases of small-pox was reduced in Copenhagen from 450 to 9; in Prussia the average was as 12 to 1; Berlin, in 1819, only 25 had died, being about 1 in 8000; Bavaria, in 11 years, only 5 had died; Anspach, the disease had been completely exterminated; Norwich, in one year the small-pox cut off more persons than any disease, except the plague; Edinburgh, similar havoc; London, in one year 13,000 died; Russia, from 1804 to 1812 there were upwards of 1,200,000 individuals vaccinated.

Mr. Marshall then stated the various causes of failure, as age of virus, want of care, bad selection, &c. &c.; and the precautions under which vaccination might be considered as a thorough barrier to the small-pox.

April 29*th.*—Mr. Faraday on Mr. Trevellyan's *recent Experiments on the Production of Sound during the Conduction of Heat.*

Mr. Trevellyan had remarked that when a heated poker was laid upon a table, so that the knob rested upon the table, but the hot part upon an interposed block of cold lead, regular musical notes were frequently produced. By extending his experiments, he found that a better form than that of a poker might be used for the hot metal: a piece of brass about four inches long, one inch and a quarter broad, and half an inch thick, should have a groove of one-eighth of an inch in width, formed down the middle of one of the broad faces, and then that face bevelled from the edges

of the groove on each side. Being now placed with the groo downwards upon a table, and shaken, it rocks to and fro, and is right condition for the experiment. It is convenient to faste brass wire, terminated by a knob, to one end of this rocker, so as act as a prolongation of an axis: it renders the whole arrangem steady and regular in its action. When this piece of metal is u instead of the poker, musical sounds are almost always produc The surface of the lead upon which it rests should be clean.

The peculiar effects exhibited in these experiments depend up the occurrence of isochronous vibrations performed by the rock When by loading the rocker these are rendered slow, they beco visible: but when they occur with sufficient rapidity they prod the necessary result, a musical note, of higher or lower pitch, as t vibrations or tappings are more or less numerous. It often happe that other and extraneous sounds, as those due to the ringing the metal, the vibration of the table, or subdivisions of the wh vibrating system, mingle with the true sound produced by the blo of the rocker; these were referred to and illustrated, and a meth shown of easily distinguishing the latter from the former: it co sisted in pressing perpendicularly with a small stick or point metal rod on the back of the rocker, exactly over the groove, so to make the vibrations quicker, but not to disturb their regularit the true sound of the beats of the rocker immediately rises in pitc and may be sometimes made to pass through an octave or more pleasure, falling again as the pressure is removed.

As the sound was evidently due to the rapid blows of the rock the only difficulty was to discover the true cause of the sustaini power by which the rocker was continued in motion, whilst a considerable difference of temperature existed between it and t block of lead beneath; this Mr. Faraday referred to the ultimate e pansion and contraction, as Professor Leslie and Mr. Trevellyan ha done generally; but he then gave a minute account of the mann in which, according to his views, such expansion and contracti could produce the effect. When the heated rocker is reposing up a horizontal ridge of lead, it touches at two points, which are al heated and therefore expanded, and form two hills; when one si of the rocker is raised, the point relieved from its contact is instant cooled by the neighbouring portions of lead; the expansion cease and the hill falls. When the rocker, therefore, is left free, t raised side descends through a greater space than that throu which it was lifted; and also to a lower level than the other side: consequence of which a momentum is given to it, which carries i centre of gravity beyond the point to which it would pass if the had been no alteration in the heights of the sustaining points. It this additional force which acts as a maintaining power; it recu twice in each vibration, *i. e.* once on each side. The force gained by the whole rocker being lifted bodily by the point which it is for the time supported, and comes into play by the si

of the rocker which is descending, having a greater space to fall through than that which is passed over by the mere force of its momentum during its previous rise. A curious consequence of this action is, that the force which really lifts the rocker is on one side of the centre of gravity, whilst the rising side of the rocker itself is on the other.

This, however, is not the only maintaining cause or mechanical force generated by the alternate expansion and contraction of the lead. If the vertical direction of the forces be put out of consideration for a time, and the two points of support be examined, it will be found that whilst the rocker is quiescent, both points (with their neighbouring parts) being heated, will expand and compress the lateral portions of the lead, until the tension of the latter is equal to their own. When one side of the rocker is raised, the point that it rested upon is instantly cooled, and therefore contracts; but as the neighbouring parts retain their tension, they move towards the contracting part, the other point of support moving with the rest. When the rocker returns in its oscillation, it reheats and re-expands the first point of support, whilst the second, now out of contact, is cooled and contracted, and the first point, therefore, moves towards the second. A necessary consequence of this mutual relation of the points is, that the one under process of heating is always moving towards the other which is under process of cooling; and, consequently, towards a perpendicular from the centre of gravity; but as it is at the same time the supporting point to the rocker, that supporting point is, by irresistible impulse, carried in a direction under and *towards* the line passing from the centre of gravity towards the earth, at the same instant that the centre of gravity of the rocker is, by the momentum of the latter, moving in the opposite direction: hence a very simple maintaining power, sufficient, whenever the rocker continues to vibrate, to compensate for the loss of force in each half of the vibration which would occur if the rocker and lead were of the same temperature. Mr. Faraday illustrated the sustaining force of the lateral motion of the points of support, by placing a rocker on a piece of lead, and the latter on a board. A pair of sugar-tongs was held tightly by the bend against the edge of the board, so that the line from the tongs towards the rocker was perpendicular to the axis of the latter. On making the limbs of the sugar-tongs vibrate in the manner of a tuning-fork, they communicated longitudinal vibrations of equal duration and number to the board, and through it to the lead and points supporting the rocker; which latter itself immediately acquired vibratory motion isochronous with the vibrations of the tongs, and by successive blows upon the lead produced sound: upon removing the rocker, and repeating the other parts of the experiment, no sound was produced.

Experiments with other metals were then made. A piece of curved silver plate being heated and placed on an iron triblet, rocked and sang in the manner of the others; this is an effect which work-

ing silversmiths have long known. The superiority of lead; as cold metal, was referred to its great expansive force by heat, co bined with its deficient conducting power, which is not a fiftl that of copper, silver, or gold; so that the heat accumulates m more at the point of contact in it, than it could do in the la metals, and produces an expansion in that respect proportiona greater.

Mr. Trevellyan's paper had been read to the Royal Societ Edinburgh, but is not yet published. Mr. Faraday stated that Trevellyan had very liberally allowed him the use of a written c

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May 6th.—Mr. Lindley *on the Pitcher Plant.*—On this ever Mr. Lindley brought before the meeting some illustrations of plants that have those remarkable appendages which botanists *Pitchers* or *Ascidia.*

He remarked that appendages in which water or fluid col have been noticed in a variety of plants; but that he did not pose, upon the present occasion, to advert to any in which pitchers do not form a striking and principal feature of the veg tion. These he stated to be the following.

Firstly, all the species of *Sarracenia,* little North Ame swamp-plants, in which the pitchers are hollow, green, se bodies, arising from the crown of the plant, and surrounding scape; they are furnished with a projecting membranous win their face, are terminated with a green leaf-like lid, and are co inside with numerous inverted hairs.

The second kind of pitcher plants was said to consist o various species of *Nepenthes,* which are found growing in the ma and ditches of China, and the damper parts of India. In thes pitchers were described as hollow bodies, similar to those of racenia, and, like them, furnished with a sort of lid, but differi having a long stalk, which in the lower half is leafy and flat, a the upper, where it joins the pitcher, cylindrical and twisted also in proceeding from the stem in the place of leaves, inste forming a cluster of pitchers, arising from the base of the sc the surface of the ground.

A third kind, a native of swamps in New Holland, the *C lotus follicularis,* was compared to Sarracenia, in regard t organization and position of its pitchers, but was stated remarkable for the presence of flat leaves of an elliptical among them.

All the foregoing were said to be either herbaceous plants, least not more than undershrubs; that is in the case of nepenthes mediate between herbaceous and shrubby. Trees or climbing were next mentioned, as sometimes having appendages to the name of pitchers might be applied, although destitute remarkable lid which exists in all the kinds previously named

Thus in the *Dischidia Rafflesiana* and *Clavata,* two plants the one in the Indian Archipelago, and the other on the c

Martaban, the pitchers are in the form of large yellowish-green bags, hanging in bunches from the slender woody stems by which the species climb to the tops of trees; and in *Marcgraavia umbellata,* and in the genus *Norantia,* the former a West Indian climbing plant, the latter small trees found in the midst of rocks and mountains in Brazil, especially in the Minas Geraes and Serra Dorada, the pitchers are small, coloured, hollow bodies, occupying the place of bracteæ, and hanging down or standing erect among the flowers.

These forms of pitchers were illustrated by highly magnified drawings, and by beautiful specimens furnished for the occasion by Dr. Wallich.

With regard to the uses for which these curious organs are destined, it was observed that a variety of opinions had been entertained, among which it was difficult to say which was most unsatisfactory. Thus Rumphius supposed that the pitcher of Nepenthes was intended as the nest of a sort of shrimp frequently found in it; Morison considered it in Sarracenia as an 'operculum divina pro- 'videntia ad obtegendam et defendendam plantam a pluviarum 'injuriis statutum." Linnæus compared Sarracenia to a water-lily growing in dry ground, and thought its pitcher was a reservoir of rain; and he supposed that in Nepenthes the pitchers were reservoirs of water, to which animals might repair in time of drought, their lid being especially destined to close up the mouth of the vessel, and thus to prevent evaporation. Sir James Smith thought that in Sarracenia the pitcher was intended for an insect-trap, because insects are often found in the water, and because the stiff inverted hairs that line it are peculiarly well adapted to prevent the escape of insects once inclosed in it; and that the putrescence of the insects was converted into the food of the plant.

The objections to these theories were obviously, that they either depend upon data which are actually false, as in the case of Linnæus, when he fancies that a plant which really grows in marshes is a native of dry situations; and that a lid, which never alters its position when once raised from the pitcher, has a power of contracting and closing up the mouth to prevent evaporation; or else upon unsupported hypothesis, as in the case of Smith's opinion, that the putrescence of insects generates food for the plant; and of others who think that the pitchers are reservoirs of water for the use of animals in dry weather. With regard to this latter supposition, it was remarked that, in Sarracenia, the water could not easily be emptied out of the pitcher except by birds; that in Nepenthes it evaporates, without being renewed, shortly after the elevation of the lid; that in Dischidia and Norantia it is not easy to conceive how the pitchers can be emptied at all; and, finally, that it is contrary to reason to suppose that Providence should make provision for an accumulation of water for the use of animals, exclusively in those places where, in consequence of the humidity of the atmosphere, or the nature of the marshy soil, such an accumulation would be of no use.

It was suggested that pitchers have doubtless different us different plants. In the Dischidias it is probable, as Dr. W has remarked, that they are reservoirs of nutriment, from whicl roots, emitted by the stem, and constantly found ramifying w them, absorb food for the general support of the individual, and in this case they become necessary in consequence of their slender twining stem being too narrow a channel of supply the subterranean roots to the leaves. With regard to Nepenthe following idea of the uses of its pitcher was offered for consider It has been discovered by Mr. Valentine, that a vast quant spiral vessels is found in the stem and petioles, a quantity so siderable that no plant has yet been noticed in which the equally abundant; now it has been ascertained by Bischoff spiral vessels convey air containing about twenty-eight per ce oxygen; and as it is well known that an accumulation of, excessive supply of, oxygen is destructive to vegetable life, is possible that the pitchers are a contrivance to enable the pl get rid of its oxygen, and may not the water that they contain been discharged by the spiral vessels themselves? It was sub that a confirmation of this opinion was apparently afforded observation of the late Dr. Jack, that the bottom of the pitch the Penang species, in the inside, is beautifully punctured, as the mouths of vessels; and also by a remark of Dr. Grahan the water in the pitchers formed in the Botanic Garden, Edinl was at first subacid, and continued to increase in acidity t whole evaporated.

It was stated that scarcely anything was, however, known exact nature of the water in pitchers; it having been only an in one instance, when Dr. Turner found the contents of Nepen contain minute crystals of superoxalate of potash. It is well that in Sarracenia the water is putrid; and in Norantia it scribed as sweet in one species, and bitter in another.

The last subject of inquiry was the nature of the pitche their analogy to the other and more common organs of veg With reference to this poiut, some remarks were made u doctrines of morphology, a subject first distinctly adverted to country, incidentally and in a very concise manner, by Mr. Br 1816, but originally conceived by Jungius in 1678, and subse explained in an admirable treatise by the celebrated poet in 1790. The sum of this doctrine is, that a plant usually of only two essentially different parts, viz., the axis (or st root) and the appendages of the axis, all of which, under w form they may appear, whether of bracteæ, calyx, corolla, s or fruit, are mere modifications of leaves. Consequently, i follow that pitchers are also leaves; an opinion that was su in Sarracenia by a reference to their evident transition fi form of leaf in *Dionæa Muscipula*, the pitcher itself being in a particular state, and the lid the lamina; in Nepen

Cephalotus by their obvious identity of nature with Sarracenia; in Dischidia by their position upon the stem, by the condition of their petiole, and by their relation to the inflorescence being the same as that of leaves; and in Norantia by their position with regard to the flowers, and their gradual transition from leaves to their most perfect state.

May 13*th*.—Mr. Brockedon *on the Passage of the Alps by Hannibal.*—Mr. Brockedon, in offering his remarks upon the passage of Hannibal across the Alps, illustrated his observations by drawings and maps.

His object was to show the errors and absurdities into which writers upon this subject had fallen by fire-side conjectures and reliance upon previous authors, who were as ignorant of the Alps as themselves, and whose chief authorities were incorrect maps. His own actual examination of above forty of the passes across the Alps, having traversed them nearly sixty times, has brought to test the impossible and impracticable routes laid down by different authors —impossible under the authority of Polybius—which, contemporary as he was with the event, and consistent as he was with himself throughout his narrative, Mr. Brockedon thought was the only authority to be relied upon. He successively exposed the fallacies of Livy, St. Simon, Folard, Fortia d'Urban, Whittaker, Laranza, and other authors who theorized upon any other pass than that of the Little St. Bernard, where alone, throughout the great chain, Mr. Brockedon observed the coincidences of times, distances, and events, as related by Polybius, could be found to have occurred.

The researches of General Robert Melville, given to the world by M. de Luc of Geneva, were the first to excite attention to the true line of the passage of the Carthaginians; the subsequent examinations of this and other routes in the Alps by Messrs. Wickham and Cramer, of Oxford, confirmed General Melville's views; and Mr. Brockedon's repeated journeys over the Little St. Bernard, and every other course across the Alps, which it was possible for Hannibal with his army to have taken, confirmed his belief in the moral certainty that it was by this pass only that the extraordinary entry of the Carthaginians into Italy was effected.

May 20*th*.—On this evening Mr. Charles John Robertson, of Worton-house, Isleworth, gave an account of his improved method of painting in water colours.

The object which the lecturer proposed to himself in the experiments which led to these improvements, was the uniting the advantages supposed to exist separately and exclusively in oil or in water colours. Oil painting has been supposed to possess greater durability, and the exclusive power of executing pictures upon a grand scale: the oil which is used as a vehicle bringing out the colours to their fullest tone, and producing a richness and mellowness of effect

of which it has been supposed water colours are incapable, w were considered, till within these few years, to be fit for little than the slight sketchy works of amateurs; and, indeed, not standing the great and rapid improvements made within the thirty years, and the beautiful specimens that have been seen annual exhibitions of the Society of Painters in water colour works painted in water have hardly yet established their just to be called paintings, being usually named drawings, to distin them from works in oil. Still, every lover of art has felt and a ciated the superior brilliancy and purity of the tints of water-c pictures. This they owe to their being painted with transp colours on a white ground, where the rays of light passing thr the diaphanous colours to the paper, are reflected back to the tator, producing a similar effect to the light reflected th a jewel from the foil at the back; while pictures in oil, painted for the most part with opaque colours, give back the from the surface, and deprive them of that fulness of tint wh possessed by water colours, in this case resembling the lig flected from the surface of a jewel which, in those of the d colour, appears nearly colourless. Now the question of dur is not so easily disposed of as a hasty adopted prejudice assert. If any collection of oil pictures be carefully examined in the highest state of preservation will be found to have su considerably, while the majority are obscured from various ca the oil itself becomes dark and opaque by time, and acts chem upon many of the colours; which again being mixed upon the ter's pallet without regard to their chemical nature, also act each other: thus the light colours become browner, and the c colours mealy; then follows the ruinous practice of cleanin varnishing, the evils of which our space will not allow us to e rate, but which are well known to those interested in the s On the other hand, pictures in water-colours, by the usual r require a glass to preserve them from the injuries of dirt and ture, and if they become stained or dirty, cannot be cleaned w the greatest risk of destruction.

The risk of breaking, the weight, and the expense of plate would necessarily limit the size, did not the inferior force of colours, by the old method, preclude the attempting a large p But our limits warn us that we must proceed to describe bri new method as proposed by Mr. Robertson, which he illu by pictures, one of them containing about forty square feet face, with whole-length figures the size of life, the produc which excited the full approbation of the audience. Mr. R son attaches the paper upon which he paints, by means and paste, to a strong linen, the back of which he def tin foil, also firmly attached by the same means, which secu part from the effects of damp, a great source of injury to p Beginning with a neutral tint, he washes in all his colour

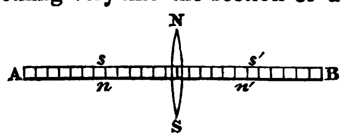
rately and unmixed, each separate colour being firmly fixed in the paper, by washing it plentifully with water till the water runs off clear, so that there is no danger of one colour tarnishing another by mixing with it; by this means the colours are so pure that he is not under the necessity of using those that are not permanent, for the sake of their brilliancy, as it is well known that a transparent colour used over another produces a much brighter third colour than when the same two colours are mingled (his colours are prepared in the usual way). When he has got his picture to the utmost degree of force that he is able by these processes, he varnishes it over in the early stages with a solution of gum tragacanth, and in the latter with a solution of isinglass in alcohol, painting over these and again varnishing, till he produces a depth of colour equal to the most powerful oil picture. By these means colours that, although permanent in themselves, may injure other colours (permanent separately) by their chemical agency, become protected from each other, and all are defended from the action of gases and vapours, as well as from injury by smoke, grease, or dirt; for the latter may, with the greatest facility, be removed by spirit of wine, which readily dissolves them, while it cannot injure, even in the slightest degree, the surface of the picture: for since isinglass can only be dissolved in spirit of wine, by being kept at the boiling point for several hours, it is evident that when used in a cold state it cannot affect the varnish. To defend the picture from injury by humidity, it may be varnished by any of the varnishes used for oil pictures, which may all be taken off again readily by alcohol, down to the exact surface of the picture. Nay, if the whole surface were painted over with oil paint and suffered to dry, it might be as easily cleaned. Amongst those present were the president and several members of the Royal Academy, who expressed the greatest interest in Mr. Robertson's principles and details.

May 27th.—Mr. Britton's *remarks on, and illustrations of, the Old Domestic Architecture of England.*—This subject was illustrated by a variety of curious prints, drawings, and models, tracing the progress of the art, from the Norman style still extant in some of our ancient buildings, down to the period when Roman architecture began to be mingled with our national works, and till it ultimately supplanted them in the buildings destined for domestic use, as well as those intended for divine worship. The new buildings at Windsor Castle, and those in and about London, were very particularly dwelt upon, as indications of the present state of domestic architecture.

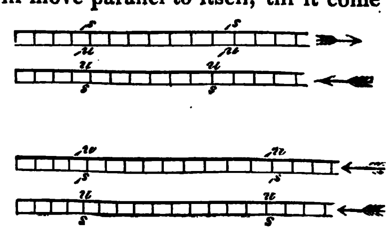
Numerous presents were placed on the library table, given by G. Bennet, Esq. They were principally from the South-Sea Islands.

June 3d.—Mr. Ritchie *on the relation between Electricity and M netism, and on Electricity as the probable origin of all the phenom of Natural and Artificial Magnetism.*—After a few general obsei tions on the nature and laws of action of voltaic electricity, Mr. Rit proceeded to illustrate the striking relations between a conducto voltaic electricity and artificial magnets. As the law accordin which the needle places itself across a conductor is easily forgot the lecturer recommended the following artificial mode of viev it, as the best for fixing it securely in the memory. Regarding sun as the visible cause of terrestrial magnetism, and conceivi current moving round the earth in the direction of the sun's a rent motion, from east to west, let a person conceive himself lool towards the east, and then lying down on his back, with his fee the east, he may consider himself as a portion of a conductor, current of positive electricity entering at his feet and passing o his head. If he now conceive a magnetic needle suspended al his chest, the north pole of this needle will arrange itself tow *his left hand,* which will always be its direction, whether he th of terrestrial or artificial electricity.

Mr. Ritchie then described the mutual action of two voltaic ductors, discovered by M. Ampère. If a voltaic conductor act magnetic needle, it may follow that two conductors will act on other, and the manner in which they will act may be imagine reasoning *à priori.* If an indefinite number of very short ma be placed transversely on a flat piece of wood, with their pol the same name, all placed in the same direction, we shall have, Mr. Ritchie, something very like the section of a conducting



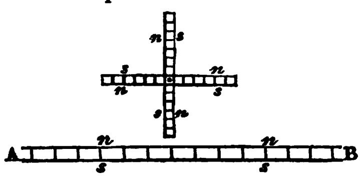
Let AB be a slip of wood, having a great number of small ma made of portions of sewing needles, cemented on the slip of with all their south poles above and their north poles below, a a small magnetic needle be suspended above this compound and the needle will obviously arrange itself as in the annexed f If two such bars be placed parallel to each other, as in the an cut, with their poles in the same direction, and if one of th moveable, it will move parallel to itself, till it come in contac



the other. This is obvious, since dissimilar poles are opposite to each other, attraction must take place. If they be placed, as in the second case, the moveable one will be repelled, and continue to move parallel to itself, till it get without the sensible repulsive action of the other.

Viewing these compound bars as metallic slips conducting voltaic electricity, it is obvious that attraction will take place when the currents move in the *same* direction, and repulsion when they move in opposite directions.

The same illustration will apply to a terminated current moveable about one of its extremities, when acted on by a straight indefinite current. Let AB be an indefinite straight conductor, and DC a terminated conductor moveable about the point C. It is obvious that in this position repulsion will take place, and the moveable conductor will turn round C, till it arrive at a position at right angles to the straight conductor. At this point the attraction of the other half of the straight conductor will act till it be brought to a parallel position. By viewing the tendency to move in every position, we clearly see that the terminated conductor has a tendency to revolve about the point C, and by a sufficiently powerful battery this revolution may be made to take place.



By substituting a magnet for the conductor the same thing takes place, as in the beautiful experiments of Mr. Faraday and M. Ampère.

A portion of a circular conductor may be viewed as a straight one, and all the phenomena of terrestrial magnetism may thus be satisfactorily explained, by supposing currents of electricity circulating about the earth from east to west. Such currents may be made to act on terminated currents, and produce all the phenomena of attraction, repulsion, and rotatory motion, as has been done by M. Ampère.

By means of an artificial globe, surrounded with coils of copper wire, Mr. Barlow has exhibited all the effects of terrestrial magnetism, on the direction and dip of the needle. Without entering into the scientific details of the theory of terrestrial magnetism, Mr. Ritchie showed, by a very simple experiment, that the old theory, which viewed the earth as a huge magnet having two poles and a middle line, was inconsistent with known facts. If a vessel of water be placed on the middle of a large magnet, and a light magnetic needle floated on the surface of the water, this needle will arrange itself in the direction of the poles of the magnet. So far it agrees

with what actually takes place on the surface of the earth. Bı needle cannot be made to remain in that position. The attrā of one of the poles always overcomes the attraction of the otheı the needle floats towards the nearest pole—a fact quite contrı what takes place when the earth acts on the same floating nee

But it may be asked, how are these currents generated? they voltaic or thermo-electric? From the constitution of our we can scarcely doubt that they belong to the latter class. The abounds with metalliferous veins, and these veins are undou of different temperatures, and, consequently, thermo-electric must take place. The rapid change in the direction of the mε equator, when approaching South America, renders this supp highly probable.

The effect of voltaic electricity in forming temporary magn exhibited; but as that was fully described in the last Numbeı Journal, we refer to it for further information. Mr. Ritc marked, in concluding, that we were fast approaching to the when all the phenomena of heat, light, electricity, &c., wou bably be referred to the same great cause, merely acting in ways; and concluded by quoting the following predictior late Professor Playfair, which we shall give in his own w ' If, on the other hand, we consider how many different lav to regulate the other phenomena of the material world, a action of impulse, cohesion, elasticity, chemical affinity, lig netism, galvanism, electricity, the existence of a princip general than any of these, and connecting all of them witl gravitation, appears highly probable.

' The discovery of this great principle may be an ho served for a future age, and science may again have to reco which are to stand on the same levels with those of Nev La Place. About such ultimate attainments it were unv sanguine and unphilosophical to despair.'

June 10*th*.—Mr. Faraday *on the Arrangements assume ticles on the surfaces of vibrating Elastic Bodies*.—Thi subject of a paper read before the Royal Society a few w of which Mr. Faraday was the author. He stated, that his reasons for bringing it forward on the present occasion a a desire to illustrate the characteristic differences between Society and the Royal Institution, in their modes of pu scientific truths; and his conviction that every thing, wh or great, originating in the latter establishment, should as soon as possible, in the possession of the members at l

When a plate or pane of glass is held horizontally b tongs griping the glass at the centre, and a violin-bow dra edge of the glass, it is made to vibrate; and sand havin viously sprinkled upon the surface of the plate, the partic themselves into regular forms, figuring forth the quiesc

the glass. These are called by Mr. Chladni, their discoverer, nodal lines. When light particles, such as scrapings from the hairs of the bow used, dust, or the powder of lycopodium, happen to be on the plate; instead of proceeding to the same quiescent lines as the sand, they accumulate at the parts in most violent agitation, forming a cloud, and at last settling down into little hemispherical heaps, having a peculiar revolving or involving motion. This determination of light powders has always embarrassed philosophers: and M. Savart has founded a theory of some peculiar modes of vibration upon it. Mr. Faraday's object was to show, that the effect is a very simple and natural one,—consisting of nothing more than currents formed in the air surrounding the plate, which, proceeding from the quiescent to the most agitated parts of the plate, then pass upwards, and in their course carry the light particles with them. Mr. Faraday explained, and demonstrated by numerous experiments, how such a current would necessarily result from the manner in which the mechanical forces of the plate are transmitted to the air. He showed that this current could be interrupted by walls of card, when the light particles took new courses. He stated that the heavy particles went to the lines of rest because the air had not force enough to carry them in its course; but that light particles, being governable by it, were taken in the opposite direction. He confirmed this view by substituting water for air, making the plate vibrate in the former fluid, and showing that the sand was then carried from the quiescent to the agitated parts, exactly as the lighter particles were in air; and further, on vibrating plates in vacuo, he found that even the lightest particles went to the lines of rest, because there was no current of air of sufficient force to sweep them in the opposite direction. Want of time prevented Mr. Faraday from entering upon the explanation of the involving heaps: but this point is fully treated of in his paper read before the Royal Society. He announced that further consideration of the subject induced him to believe he should be able to account, by the same principles, combined with the cohesive power of fluids, for the peculiar and hitherto unexplained crispations which occur on water and other fluids lying upon a vibrating plate.

This being the last evening-meeting of the season, Mr. Faraday, on the part of the committee, took leave of the members, after earnestly exhorting them to use both individual and conjoined exertions to aid the prosperity of future seasons. In the library was placed a beautiful portrait of Sir Humphry Davy, of the full size, copied by W. Pickersgill, jun., from the portrait by Sir Thomas Lawrence.

parts sometimes rise to the same height, in which case they fo *verticillus*, and sometimes to different heights. Although the l case is much more frequent than the former, the authors who written on the subject generally speak of these flowers as comp as *verticilli*, probably because the difference of height betwee various parts of each system is so small as to appear to unworthy of notice. But M. Jussieu, by taking a minute acco these differences, seeks to establish that the most general law tive to the disposition of the leaves of a branch may be equall plied to the parts of the flower. Let us suppose the parts ins in different points of a spiral line turning on the conic sp (*noyau*) of the flower. Let us divide the surface of the cone five equal parts, by so many lines let fall from the sumn the base, by which each spiral turn will be cut by these lines a points. Let us then suppose an insertion at any of the points tersection, and place them upon these points alternately (*de d* after two spiral turns we shall find the sixth insertion si directly over the first, and the first five will form what Bonnet a quincunx. When the parts are large enough for the b or edges to pass each other, they will cover or lap over each ot such a manner as to form two exterior, two interior, and one mediary, that is to say, covering on one side and covered on the The two exteriors will be placed first and second, the interm third, and the two interiors fourth and fifth. It is necessary member these characteristics, because they serve to point o order of insertion where the difference of height is too small to as a guide. When the parts are large enough to lap ove other, not only with their borders or edges, but by the great of their surface, one will envelope, or lap over, two, two ove three over four, and four over five. This is the inflore which has been called *enveloping*, and which differs from th cuncial only by the enlargement of the parts. The parts of cunx in the flower are generally placed alternately with those two quincunces which are immediately above and below it. difficult to account for this disposition on the hypothesis of a spiral, but it is perfectly intelligible if we admit the existen second spiral entirely similar to the first, and revolving on th cone, but commencing from the opposite point of the base worms of a double screw. It is upon these two spirals t concentric spirals are inserted alternately. This suppos justified by observing the flowers on which the parts which a tiples of five, or some other number, alternate among thems various rows; it is evident that this alternation results from t being inserted at equal intervals upon several spiral parallel petals of the cactus, the stamina and fruits of several of the lias and ranunculi, furnish examples of this. The dispositio scales in the common pine cone is also an illustration, if no tive proof, of this.

From what has been said, the five folioli of the calyx of a flower may be considered as placed on the first spiral. At the point corresponding to that of the sixth insertion, but on the second spiral, the quincunx of the petals will commence, which will also be disposed upon two spiral turns, nearer to the summit than those which bear the folioli. When the system of the petals is finished, that of the stamina will begin, and form a third quincunx, situated on the same spiral as that of folioli, but on the fifth and sixth turns. Lastly comes the quincunx of the ovaries, which will be inserted on the seventh and eighth turns of the second spiral. It has here been supposed that each spiral begins to form the first part of one quincunx at the moment at which the other spiral completes another; but this regularity does not always occur. Thus the second, instead of bearing the first petal at the point corresponding with that which a sixth foliolus would occupy, may bear it at the point corresponding with the seventh, eighth, ninth, or tenth; whence there are four other combinations possible; and although the alteration of the parts in the two successive quincunces, and the corresponding opposition of those of every other quincunx, is apparently unaltered, the regularity is less perfect, because, although a stamen will always be found corresponding to a foliolus of the calyx, and an ovary to a petal, it will not correspond with the foliolus and petal bearing the same number. The nearer the spiral rises towards the summit of the cone, the more its turns contract and approach each other. The lower, therefore, a quincunx is situated in a flower, the more its parts are separated from each other. This remark may give rise to several deductions. In the first place the principles just laid down will be the less easily recognised in proportion as their exemplification is sought for in a higher or more interior quincunx, because the contraction of the spiral turns tends to give it the appearance of a *verticillus*, and the slightest deviation in the insertion of a part will tend to change its apparent order: it is therefore in the calyx that these principles may most frequently be verified. The inequality of height in the insertion of the petals can rarely be observed, except by their situation being a little more exterior and interior, and even this can rarely be perceived in the flower when open; in the bud it is more perceptible. The laws of the quincunx, being once admitted for the calyx and corolla, should also by analogy be admitted for the stamina and the ovaries, although the latter more generally present the appearance of a perfect *verticillus*. Several instances, however, may be found in which this appearance does not exist; added to which the developement of the parts of each of these pretended *verticilli* is much less contemporaneous than is generally supposed. This developement, indeed, would naturally be slower in the highest parts, which must be impeded by want of space; whence it arises that total or partial abortions are more frequent in proportion as the quincunx is in a more elevated situation in the flower. Thus abortions are rare in the calyx, less so in the corolla, still less so in the

stamina, and frequent in the ovaries, the number of which is less than that of the other parts, and the unequal developem which may be constantly observed both during and after its pr towards maturity. Thus the passage from regularity to irregula flowers becomes insensibly established. These irregularities hardly be explained on the supposition of the parts being exa *ticilli*, and therefore placed in conditions equally favoura developement. In fact, no examples of irregularity are fo flowers of the valvulary or twisted efflorescence, which inv indicates the disposition of the parts in *verticilli*. In a cunx, on the contrary, it is clear that the upper parts are place situation more unfavourable to their developement. This is cially true with respect to the ovaries, because the action res from the relative situation of the parts has fuller force from being no quincunx above it. This tendency to irregularity flowers in which the parts are disposed in quincunces, is not observed when the two halves of the conic spindle are in equally favourable conditions, which is the case when the a this cone has a direction according to the elongation of the ped But when the flower rests on the peduncle by an oblique base, its halves is situated higher than the other with relation to th of the plant, and is therefore in a situation more likely to pr abortions. This obliquity of the base exists in the majority o gular flowers, particularly in those in which the inequality of t halves is very decided, and which are called *didynamiæ*. causes of irregularity will be much greater, if, instead of sup the spiral constructed on the circle which forms the base of th spindle, we suppose it constructed on a section which is obli the axis, that is to say, on an ellipsis. In this case, every spir ascending and descending alternately as respects the axis, the of the insertions will no longer have an exact reference to heights, and thence will result an order apparently differing fr quincunx. The calyx of the *antirrhinum majus*, the in of which is extremely oblique, affords an example of this. M. J having thus laid down his position, proceeds to exemplify it merous examples, illustrated by plates; but it is unnecessary them here, as the above explanation of his theory will be suffic enable our botanical readers to verify or refute his ingenious thesis. We shall take care to make known the report which made to the Academy on the subject.

Chemistry.

Perchloric Acid—Perchlorates—a test for Potassa and S On the 25th of April, M. Serullas communicated the partic some experiments which he had recently made on this aci sidered as a re-agent, by means of which to distinguish and s soda from potash, either alone or combined with other acid was led to make these experiments from observing the great

ence in the degree in which the perchlorate of potash and the perchlorate of soda are soluble. The first, at a temperature of 15° C. (59° F.), requires more than sixty times its weight of water to dissolve it, while the latter is remarkably deliquescent, and consequently easily soluble, not only in water, but even in alcohol. In order, therefore, to distinguish and separate these two alkalies, it occurred to M. Serullas that it would be expedient to ascertain the possibility of producing, in the same liquid, a salt from potash, which would be almost insoluble, and one from soda which would be very soluble. Neither hydrochlorate of platina, tartaric acid, nor hydrofluosilicic acid, all of which may be employed to precipitate potash and soda, afforded sufficiently accurate means of obtaining the desired result; but M. Serullas ascertained that if perchloric acid be poured, drop by drop, into a solution of soda and potash mixed, a perchlorate of potash is instantly precipitated; the perchlorate of soda (or the soda itself, if there be not an excess of acid) remains in the liquid, whence it may be separated by concentrated alcohol, which will precipitate at the same time the small quantity of perchlorate of potash which may remain. A solution of perchlorate of soda, to which potash is carefully added, will instantly precipitate a perchlorate of potash, the soda becomes free, and may be separated by alcohol. Owing to this great difference between the solubility of the perchlorate of potash and that of every other salt having the same base, it is easy to ascertain the existence of potash, either free or combined with other acids, in any saline solution, as the smallest quantity of perchloric acid occasions a precipitation of perchlorate of potash, while the other acids are rendered free, and may be isolated by alcohol. This experiment has been tried with the sulphate, nitrate, chlorate, bromate, muriate, and hydrobromate of potash. By this mode of proceeding the simultaneous existence of soda and potash may always be ascertained, and it will also be easy to examine the nature of the acid primitively combined with the former, as it may always be isolated by concentrated alcohol. There will also be a great advantage in employing the perchlorate of barytes, and that of silver, (both of which are very soluble,) in cases of combinations of soda and potash with sulphuric acid or hydrochloric acid, as, in both cases, by means of alcohol, all the perchlorate of soda may be obtained on the one hand, and on the other, all the perchlorate of potash, with the sulphate of barytes, or the chlorure of silver, from which the perchlorate of potash may easily be removed by washing it in warm water. The general results of the above experiments are—

1. That perchloric acid forms, with potash, a salt soluble with great difficulty, and requiring for its solution sixty times its own weight of water, at a temperature of 59° F.

2. That soda forms, with the same acid, a salt very deliquescent, and consequently easily soluble, either in water or even in the most highly concentrated alcohol. This fact was previously unknown.

3. That these opposite qualities of the two compositions
the means of separating soda and potash when in solution tog
the former generating a perchlorate easily soluble in alcoho
the latter a perchlorate absolutely insoluble in that liquid.

4. That any acid whatever, which was primitively combine
potash, will always be separated and set at liberty by perchlori

Production of Perchlorate from Chlorate of Potassa.—C
23d of May, M. Serullas communicated the result of his
vations on the change of chlorate of potash into oxychlorate
chlorate) of the same base, by the action of heat, and on
method of obtaining oxychloric or perchloric acid. The
rity of the phenomena manifested in the production of ox
rate of potash, with those observed in the production of oxy
acid, led M. Serullas to suppose that the simple action of
temperature, maintained within certain limits, would convert cl
into oxychlorate of potash, by combining part of the oxyge
the chlorate remaining undecomposed. He had long since ob
that chlorate of potash, decomposed by fire, left a saline
not easily soluble, and removable with difficulty from the tu
which the experiment was made, but he concluded that form
mists, in establishing that this residue consisted only of ch
had taken notice of all the effects produced by heat at d
periods of the operation; and, therefore, had not directed his at
to that point until accidentally led to do so in the course
experiments on oxychloric acid. He observed that when chl
potash is heated in a crucible, it first melts, and then boils, at
state there is an escape of oxygen. When the heat is re
sparingly, after a certain duration of the ebullition, this es
oxygen ceases, unless the temperature be raised; if the op
be then suspended, and the solution be filtered while hot,
derable quantity of oxychlorate will be obtained, when it c
small brilliant crystals: 40 parts of chlorate furnished 17·5
oxychlorate. M. Serullas has established that Stadion, and
other chemists, were wrong in supposing that oxychlorate of
is decomposed at 392° Fahr.: it requires at least 752° Fahr

In order to obtain oxychloric acid, oxychlorate of pota
be boiled with silicated hydrofluoric acid, in great pa
porated, so as to obtain, when cool, a more abundant de
the gelatinous fluosilicate of potash; this must be filtere
evaporated, left to cool, again filtered, concentrated in
sule, and then distilled in a small retort. In order to pr
the small quantity of fluosilicate of potash and of oxy
which may exist in the oxychloric acid, it is only necessary
into it a little concentrated alcohol, filter it, dilute it wit
and then let it evaporate. The oxychlorate of potash
65·725 parts in 100 of oxychloric acid. This simple mean
curing the acid is highly important, as from M. Serullas'

of its utility in separating soda from potash, as above-mentioned, it will necessarily become in great request.

Action of Vegetable Substances, Gum, Sugar, &c. in contact with Metallic Oxides.—On the 2d May, M. Becquerel communicated to the Academy a very interesting paper on carbonate of lime in crystals, and on the simultaneous action of saccharine and mucilaginous matters upon the oxides of certain metals, obtained through the medium of alkalies and earths. M. Becquerel has, for a considerable period, directed his attention to the means of submitting organic substances to the action of electric currents, with the view of ascertaining the causes of some of the phenomena observable in those substances, particularly that of fermentation. It was already known, from the experiments of Cruikshank and Daniell, that on exposing a solution of sugar and lime in water to the action of the atmosphere, small crystals of carbonated lime are produced on the surface; but the cause of this phenomenon was entirely unknown, although it was supposed that the carbonic acid might perhaps be supplied by the atmosphere. M. Becquerel, however, has, by means of the following experiment, ascertained the real source of the acid. He plunged into a wide-mouthed bottle, filled with barytes water, two tubes, (the lower parts of which were stopped with moistened barytes,) filled, the one with a solution of lime and sugar, and the other with a solution of sulphate of copper. The liquid contained in the first tube was connected with the positive pole of a voltaic pile, by means of a plate of platina, and that in the second tube with the negative pole, by means of a plate of copper. The moment this communication was established, the sulphate of copper was observed to be decomposed, the copper was precipitated in a metallic state on the copper plate, the sulphuric acid was absorbed by the barytes, and the oxygen was transported to the positive pole; where, by a re-action on the carbon of the sugar, it produced carbonic acid, which was immediately combined with the lime. After the lapse of some days, small prismatic crystals of carbonate of lime were observed on the plate of platina, and continued to increase as long as there remained any lime in the solution. Gum, the component parts of which are nearly similar to those of sugar, produced the same effect. In both cases, those portions of the vegetable substance which do not tend to the production of the carbonic acid, or of the water of crystallization of the carbonate, are converted into acetic acid. M. Becquerel was next led to examine the simultaneous action of saccharine and mucilaginous substances upon the metallic oxides, through the medium of the alkalies and the earths. If hydrate of copper be acted on by water and lime, with the aid of heat, it becomes black, and probably passes into an anhydrous state; but if a very small quantity of sugar be added, a portion of the oxide is dissolved, and the liquid assumes a beautiful blue tint, similar to that of a solution of oxide of copper in ammoniac.

Honey and sugar of milk have the same properties, which,
ever, have never been observed, except in saccharine subst
Potash and soda may be substituted for lime in this exper
with a similar effect, except that their faculty of diss
is greater, whereas that of barytes and strontia is much
Gum does not produce the same effect as sugar: that subs
when dissolved by water, is not precipitated by the alkalie
earths which we have just mentioned, but if a deutoxi
copper, in a state of hydrate, be added, a flaky insoluble prec
of gum and oxide of copper is formed. When there exists i
solution a small quantity of saccharine matter in addition, it
immediately on the excess of oxide, and of copper, which has
added, dissolves it, and gives a blue colour to the solutior
order, therefore, to detect the existence of gum and sacc
matter in any substance which contains both, it is sufficient
potash and caustic lime to the solution, and then apply hyd
copper to it. The mucilage found in a decoction of linsee
duces the same effects as gum; and as the solution becomes s
tinged with blue, it is evident that it contains saccharine r
If the solution be acted on by heat, the effects are different.
solution of sugar, potash, and deutoxide of copper, in wat
heated to the boiling temperature, the blue colour changes s
sively to green, yellow, orange, and finally to red, and then
deutoxide is changed into protoxide. If oxide of copper b
added gradually, until there is no longer any protoxide form
the sugar is decomposed, and nothing remains in the soluti
carbonate of potash and a small quantity of acetate of th
base. The saccharine matter of milk, which, when cold, a
copper and potash in the same manner as common sugar, a
ferently when heated. The deutoxide of copper passes fi
state of protoxide, and is then reduced to a metallic state.
oxides of gold, silver, and platina, submitted to the same t
the oxide of copper, are reduced to a metallic state, while the
of iron, zinc, and cobalt do not undergo any change.
deutoxide of mercury is reduced to a metallic state by pota
the saccharine matter of milk; it then, in consequence of th
which is interposed between the parts, presents itself un
form of paste. Under this form, the mercury may be app
glass without the necessity of using tinfoil; it is sufficient to
the paste in a very thin layer, and heat the glass slightly, to
the water which is interposed. Lime, barytes, and strontia
acting by means of heat on the deutoxide of copper and sac
matter, do not form compositions similar to those of the
Lime, for instance, does not convert the deutoxide into a pr
or a metallic state; it occasions a precipitate of an orange
colour, formed of the protoxide of copper and lime. In th
manner, proto-cuprates of barytes and strontia are preci
These are the principal results of M. Becquerel's experiments

have considerable importance, as showing the intimate connection between the electric and chemical systems.

Oxides of Barium.—On the 13th June, M. Despretz stated that the hydrate of barytes, which has been generally said to resist heat, is decomposed at a sustained red heat. This fact, which completes the series of experiments tried with lime, magnesia, and strontium, proves that no oxide of barium will retain water at a high temperature.

Azote and Iron.—At the same meeting, M. Despretz also stated, that iron, at a red heat, subjected to the action of azote gas, is sensibly increased in weight; it disengages azote when dissolved in the acids.

Sulphates.—M. Despretz also remarked, that all the sulphates which are not liable to be decomposed by heat alone, disengage sulphur, when acted on by carbon or hydrogen gas at a strong red heat.

Geodesy.

Heights of the Pyrenees.—On the 4th April, M. Puissant presented to the Academy a report on a memoir by M. Coraboeuf, entitled 'Sur les Opérations Géodésiques des Pyrenées et sur la Comparaison du Niveau des deux Mers.' The object of this paper was to fix with certainty the heights of several of the summits of the Pyrenees, and to determine the long-contested question as to whether the waters of the Mediterranean are precisely on the same level as those of the ocean—both being, of course, considered as in a state of perfect tranquillity. The report stated that the mode of operation, adopted by M. Coraboeuf and his associates, was such as almost to preclude the possibility of any important error existing in his conclusions. His trigonometrical observations were always made in the most favourable state of the atmosphere, and the calculations resulting from them worked with scrupulous fidelity. The results obtained by M. Coraboeuf generally coincide with those mentioned in the 'Base du Système métrique décimal'—the greatest difference being 0^m 24 on a distance of 9514^m 78, and that but in one instance, the distance from Espira to the southern point of the base of Perpignan. In calculating the heights, M. Coraboeuf made use of two tables, which he has annexed to his memoir—the one being the height above the Mediterranean, as observed by him on the eastern side, and the other the height above the ocean on the western side. The medium between these two calculations has been adopted as the definitive height. The greatest difference found between the two heights is that of Gardan de Montagu, which varies 2^m 64; but this is to be accounted for by the great difference of the level between

this point, which is situated very low, and the Crabère, the al
height of which is 2634^m; because, in such a case, the variab
the refraction sensibly affects the zenithal distances employed
calculation of the differences of the levels. This has induc
Corabœuf to adopt the summit of the Crabère as the point of
parison between the levels of the sea and ocean. His adme
ments were made by the southern direction of the intersection
triangle, by the northern ditto, and by the diagonals; the
result of these admeasurements gives, for the height of the
bère . . above the Mediterranean . 2633^m 50
above the Ocean . . 2632 77

Difference . . 0^m 73

Another series of calculations proves that the greatest pro
error in the admeasurements is, for the southern, 1^m 860; fo
northern, 1^m 421; and, for the diagonals, 1^m 416;—so that, e
there be any difference in the level of the Mediterranean an
ocean, it must be less than one of the above errors, since, acco
to the theory, the probability of that extreme error existing is
150:000. The principal of the absolute heights determin
metres are the following:—

	m		
Bugarach . . .	1230.64	Pic d'Ani . . .	2
Canigou	2785.23	Orhi	2
Pic du Col de Liouses .	2831.61	Pic du Midi de Bigorre .	2
Pic Oriental du Col Rouge	2805.81	Troumouse . . .	3
Pic d'Appi, St. Barthelemy .	2348.83	Montespé . . .	1
Moncal	3079.51	Maupas . . .	3
Lissérateca . .	1408.58		

In a subsequent part of the memoir, M. Corabœuf has rec
an error of Delambre, who had supposed that there existed a di
ence of 3 toises in the height of the summit of Salces, as tak
Montjoin and at the marsh of Leucate. The fact is, that the h
of the point above the Mediterranean is . . 362.26 toises.
and above the Ocean . . . . 361.81 „

Difference . . 45

Thus affording an additional proof that the two surfaces ma
considered as forming one level. This work will form part of
new geometrical description of France; and from its lucid de
minute calculations, and important geodesical researches, was
sidered by the Academy as deserving of insertion in the 'Rec
des Mémoires des Savans Etrangers.'

Geology.

Terrains Tertiaires.—On the 26th March, M. Reboul, a co
sponding member of the Institute, read a memoir, the objec

which was to prove that the marine deposits of the Mediterranean basins of the departments of the Herault and the Aude correspond, as to their position, with the coarse calcareous (*calcaire gros*) formation of Paris, and not with the upper marine sands and freestone. In this opinion, which is contrary to that of most geologists, M. Reboul stated that he had been confirmed by a comparison of the fossils in the two basins. The lower marine deposit of the south of France is, like that of the Seine, covered with the gypseous and siliceous *calcaire* of fresh water (*eau douce*), and in both basins an upper, or rather mixed, marine deposit succeeds the fresh water deposit. The essential difference between the lower deposits of the south and those of the Seine is, that the former are free from all mixture of fresh water fossils, and at the same time from concretions of silex, of saline deposits, and even from compact, fine-grained rocks. All these various productions are, on the contrary, found in the upper or mixed earth of this basin, which forms a stratum over the fresh-water deposits, or rather which is subordinate to them. In the basin of Paris the same productions are found in abundance in the upper stratum, but they are also found, though in a smaller proportion, in the lower stratum. As the fresh water deposits have, in this basin, preceded, or at least been associated, with the first sediments of the *calcaire gros*, M. Reboul distinguishes basins of this kind by the name of *prolymnéens* and by that of *metalymnéens*—those where the fresh water does not appear to have penetrated until after the completion of the deposits of the first epoch; such are those of the Aude, the Oise, and the Herault. The comparison of the sediments of these two species of basins opens a new field of inquiry for geologists.

On the Chalk Formations of the South of France.—On the 25th of April, M. Brogniart read to the Academy a most interesting report on a memoir by M. Dufresnoy, entitled 'Des Caractères particuliers que présente le terrain de craie dans le Sud de France et sur les pentes des Pyrenées.' The generality of the world, and even many eminent geologists, have considered that they were fully acquainted with the nature of chalk, from observing in the quarries its immense masses without any distinct stratification, and its beds interrupted by flint stones; but, having confined their attention to the external and mineralogical characters of chalk, and neglected its geological peculiarities, they have yet to learn that strata, which have not the external character of white chalk with black silex, may and do belong to the same period and the same geological formation, because they possess the same peculiarities which, in the deposits of white chalk, form the true geological characteristics. There are three modes of determining the rank which any deposit occupies in the series of which the shell of the globe is composed.—1st. The nature of the strata which are constantly found above or below it—this is the geological characteristic; 2dly, the nature

of the rock or stratum itself and the minerals which accompan this is the *mineralogical* characteristic; and 3d, the organic re contained in it—this is the *zoological* or *organic* characteristic these, the second is the least certain. By chalk deposits, in a geol sense, therefore, we mean not merely those composed of white but such as occupy the same position in the beds of the glc chalk usually occupies; which contain the same species of o remains; but which may or may not present the same mineral characters. Hence chalk formations (*terrains crétacés*) may be and compact, yellow and compact, in masses or in strata, w without silex, and even wholly composed of sand and freestone, out containing any mineralogical chalk, and even scarcely any nate of lime. Hence it is that it has been hitherto supposed that were no chalk formatious in the south of France and the Pyre

M. Dufresnoy, by his observations, has established three se facts:—1st. He has recognized these formations in parts of I and Spain in which their existence was hitherto unknown; 2d has shown that they contain mineral masses which were sup wholly foreign to them; and, 3dly, he has, on the one hand mented the number of their zoological characteristics, and, other, has diminished the negative importance hitherto attached absence of certain shells. In the south of France these form have been recognized as forming a subterranean valley, the no and southern borders of which show themselves by hilloc mounds of earth, separated from each other, but tracing, by disposition, two zones from east to west. The northern zon mences in the south of La Vendée, near Rochefort and Royan extends to the foot of the maritime Alps. The southern zon on the northern declivity of the Pyrenees, commencing fro eastern extremity of the Corbières, and extending as far as Ba in a narrow band. At Bayonne it becomes wider, and, en Spain, extends to Cardonne. The valley inclosed by these zc almost entirely filled with *terrain tertiaire* and alluvial earths Dufresnoy remarks that, by a general and almost regular ele of the granite chain of the Pyrenees, chalk formations been carried to a great height, acquiring a compact textu a black colour: Mont Perdu, a summit 3500 metres above the l the sea, belongs to this class of chalk formations. Another ele that of the ophites, has also, though in a much smaller d deranged the horizontal position of these deposits; but eruptions of ophite, to which this derangement is probably have been much less abundant on the southern than the no base of the Pyrenean chain, the chalky formations have been less deranged from their position on the Spanish than on the I side. The proofs which M. Dufresnoy gives of the nature of formations are quite satisfactory: indeed they have even prov existence in France of a group of chalk deposits (the *Weald* g which had previously been only observed in England. The

deposits of the north of Europe are placed under the *terrains tertiaires*, and over those known by the name of *epiolithic* (upper and middle oolite), because they appear to form the upper part of the great oolitic mass of the European Jurassic earth. The parts in the south of France, in which they have appeared covered with the *terrains tertiaires*, are the Landes, Medoe, the environs of Bordeaux, and St. Paulet, near the Pont St. Esprit: those in which the deposits under the chalk may be referred to the epiolithic group, are much more numerous, chiefly near Rochefort and St. Jean d'Angely.

The principal difference of the physical structure of these chalk formations from those of the north of Europe is, that they are generally in oblique strata, which may be partly occasioned by the elevation of the crystalline rocks, which constitute the Pyrenean chain, lifting up part of the layers of chalk in greater or smaller angles. The mineralogical characters of these deposits in the south of France are, in some respects, similar and in others different from those of the north of Europe; but these differences are of less importance, inasmuch as sedimentary rocks, to which these belong, can never offer those decided and uniform mineralogical characters which distinguish the crystallized rocks: for the latter are formed under the influence of chemical composition, an invariable principle of nature; whereas the former may almost be said to be mechanically constructed, under circumstances perpetually varying and subjected to no certain rule. The chalk formations of the north of Europe are composed (commencing from the surface) of white chalk, a grayish and friable rock called tufous chalk, a sandy rock, filled with green particles, and called *glauconie crayeuse*, and frequently of a sandy and ferruginous rock. Under this last rock there has been observed in England, and particularly in Sussex, a remarkable deposit of shells and fossil animals of the lake and river species: this is called the *groupe Veldien*. Sand, tolerably pure freestone, some metallic combinations of hydroxidated iron, and pyrites, are also observed in these earths. M. Dufresnoy has ascertained that the greater part of these substances are found in the chalk deposits of the south of France; and particularly that the *groupe Veldien*, supposed to be peculiar to Sussex, exists at the base of the Montagne d'Angoulême and at La Grasse. Although this deposit is less distinct in France than in England, it is easily recognized by the argilo-calcareous nature of its rock, by its position, and by its lacustrine shells (*melanie*, *paludinis*). The great difference in the mineralogical character of these deposits from those of the north of Europe is the almost total absence of white chalk, which has either never been deposited or else been carried off. The formation generally begins with the tufous chalk. Another very remarkable difference is, that in some cases (particularly at St. Froult, near Rochefort), they contain masses of gypsum, with its accompanying sulphur: by the

derangement of the beds, these masses appear to have duced themselves from below, and been developed there. rine salt is also found in these earths ; and M. Dufresnoy su that the famous bed of sea salt at Cardonne in Catalonia ou be classed among the chalk beds. But it is in the zool characteristics of these formations of the south of France th Dufresnoy has found the most distinct proofs of his theory, the same time, the most remarkable anomalies. M. Dufres addition to the belemnites, ammonites, and other fossils pec chalk, has found in these earths the bulla, the cypræa, the n —several kinds of the Venus, the Lucina, the crassatella t the neutina perversa, and other fossils, which had hitherto only found in the *terrains tertiaires*. This would, at first appear to materially lessen the reliance to be placed on the gical characteristics of strata; but it must be considered t judging of the character of a stratum from its zoological cha istics, there are four points to be specially examined.—1. The difference of the species of fossils. 2. The geographical posi the bed: this is important, because it is to be supposed difference of latitude produced the same difference in zoologic ductions of the ancient as of the modern world. 3. The p of the different species in the strata. 4. The relative nur the species, which are characteristics of the stratum in tion, and of those of which the geognostic position appe anomaly.

In the present case, M. Dufresnoy informs us that the anc or *littoral* fossils (by which we mean those usually found *terrains tertiaires*) are assembled in layers distinct from thos contain the *pelagian* fossils (as we may call those fossil are characteristic of chalk), and appear the results of a deposit. We might thence be led to suppose that, while a tate of chalky limestone, enveloping the belemnites, ammoni other pelagian fossils, was forming at the bottom of the de a calcareous earth, enveloping the cerites, the ampullæ, an molluscæ which could inhabit shallow waters, was simulta forming itself near the shores, and in the shallow waters ; the chalk and the *terrains tertiaires* would, according hypothesis, have been formed nearly at the same time, and same seas, but at different depths. Thus the fossils sir though rarely identical with, those of the *terrains tertiaires*, v the littoral fossils of the seas at the epoch of the formatio chalk. This would throw light on the nature of the Maestricht, which is so different from all others, and n supported by the presence, in that bed, of the metosa animal which, if a marine animal at all, could only have li the shores. But the concurring observations of differe establishing that the chalk deposits and the *terrains tertiair* to two distinct and probably remote epochs, are too num admit of our adopting the above hypothesis. As, howeve

existence of the two species of fossils in the chalk formations discovered by M. Dufresnoy in the south of France appears to break down the distinct line of separation which exists between the *terrains tertiaires* and the chalk in the north of Europe, we must have recourse to the consideration of the relative number of the two species of fossils, in order to see whether the anomaly and confusion are so great as might be feared. The number of species of shells and zoophytes which M. Dufresnoy has distinguished in these strata is 124, of which 110 are determinable as *genera*. Of these there appear to be only the following five which distinctly belong to the *terrains tertiaires*, as well as to the chalk strata:—*Cardium aviculare*, *Crassatella tumida*, *Cerithium diaboli*, *Nerita perversa*, and *Turbinallia elliptica*. There are about ten others, to which M. Dufresnoy has not been able to assign names, but which he considers to be identical with some of the species belonging to the *terrains tertiaires*. Thus, at the outside, there are but 15 out of 124 which belong to the two species of deposits; the 209 others have always been recognised as belonging distinctly to chalk formations. M. Brogniart then proceeds to establish, in an elaborate but lucid line of argument, that a slight anomaly of this kind cannot diminish the weight to be attached to zoological characteristics in determining the nature and epoch of a particular deposit. He remarks that it is certain that every new deposit of earth must have been occasioned by some extraordinary convulsion of nature; and that all experience shows us that the animals existing at one epoch differed so materially from those existing at another, as to enable us to distinguish, by their organic remains, the relative epochs of the formation of each deposit; but it does not therefore follow that in each of those convulsions of nature the whole of the then existing species of animals were so completely annihilated as to prevent any of them surviving or re-appearing in the succeeding epoch, in which case the admixture of the different species of fossils will be accounted for; only, as in this case, the number of those belonging to a preceding epoch will bear a very inconsiderable proportion to that of those which properly characterise the epoch under examination. In support of his opinion he cites the chalk formation discovered by M. Merton in 1828, in New Jersey and Maryland, which contains fossil remains similar, though not identical, to those of the chalk of Europe, and also several of those which we have called *littoral*, and attributed to the *terrains tertiaires*. Hence he concludes that the zoological characteristics of strata form the surest guides as to their nature and epoch, although their geognostic and mineralogical characteristics may also be taken into consideration as additional evidence. On these grounds, he considers that M. Dufresnoy has fully proved the existence of the chalk formations in the south of France—a discovery which is not only important as a matter of information on the structure of that part of the globe, but as affording a guide for useful mineralogical researches, founded

on the knowledge we possess of what substances are found bei chalk, though never either above or in them. The Memc M. Dufresnoy was ordered to be inserted in the 'Recuei Mémoires des Savans Etrangers.'

Boring the Earth.—On the 20th of June a letter was read M. Jobard, of Brussels, announcing that he had brought to p tion a new machine for boring the earth to any depth, and th any soil. He stated that his plan had been tried with the gr success in the neighbourhood of Marienburg, where he had r attained a depth of seventy-five feet, through an inclined rc phylade, mixed with argillaceous flints. By a process some similar, though less perfect, wells have been dug in China to a of from 2000 to 2800 feet, through solid rock. M. Jobard pates the greatest advantages to geognosy from his discovery with the usual enthusiasm of projectors, looks forward with dence to the period (not far distant) when we shall be a acquainted with the centre of the earth as we now are w surface.

The Lesser Atlas.—At the same meeting M. Cordier con cated some geological observations made by M. Rozet in M. Rozet is now of opinion that the earths which he had fc considered as *terrains de transition* are, in fact, to be classed those belonging to the epoch of the lias and the calcareous gr The most elevated summit of that part of the lower Atlas vis M. Rozet, and measured with the assistance of the baromet 1399 metres (4590 feet) above the level of the Mediterranea

Medical Science.

Cure of Fever.—On the 11th of April, M. Rousseau anı to the Academy, that in three distinct cases of recent occı fever had been completely cured by a few doses, of a drachı of the powder of holly-leaves, diluted with half a glass oı The Academy directed the Medical Committee of the *Pr thyon* to take cognizance of the cases.

On the 23d of May, M. Deleschamps, a young chemist, anı that he had succeeded in obtaining a new vegetable matter bark of holly, to which he had given the name of *ilicine*, ar may be substituted for quinia in the treatment of inte fevers. It will be recollected that an extract of the bark o called *salicine*, has already been suggested as a substitute phate of quinia; should experience prove that the qua these two matters are at all comparable to those of quir low price will render the discoveries highly important to t classes.

Lithotrity.—On the 18th of April, Dr. Civiale read a memoir on the diseases of the bladder, in which, after arguing on the general advantages of lithotrity, he expressed his opinion that, in cases where that mode of operation is absolutely impracticable, and it therefore becomes necessary to cut into the bladder, the hypogastric operation, as now simplified, is generally preferable to those by the rectum or the perinæum. He detailed at great length the particulars of a case in which he had successfully operated in that manner on a Russian nobleman, who had been suffering the most intense agony for more than eight years. The irritation and inflammation of the parts were so great as to render lithotrity impossible, and even excision quite a forlorn hope. The operation was, however, performed, and the patient perfectly cured in twenty-eight days. From the details of this case he draws the conclusions—that the *cystotomie sus-pubienne* may be performed in cases which at first sight appear most opposed to it; and that the passage of urine through the wound is no serious obstacle to its cicatrisation. He also takes occasion particularly to advise all medical men to carefully examine, in each case submitted to them, whether the inflammation of the urethra be the result of local irritation of the urethra itself, or sympathetic, and arising from the diseased state of the bladder, as in the latter case nothing can be done to relieve it until the primary cause is removed by the abstraction of the stone. At the meeting on the 25th of April, M. Larrey read a report on a memoir sent to the Academy some time since by Dr. Civiale, in which he gave the latter great credit for his perseverance in bringing lithotrity to perfection, but regretted that his anxiety to support his favourite theory had induced him to record only the favourable cases, and remain silent on those in which the operation had terminated unfavourably. The reporter said that the official reports of the hospitals proved that the number of patients who have died after being operated on by lithotrity is, in proportion, as great as that of those who have not survived the operation of cutting out the stone. At the subsequent meeting, Dr. Civiale questioned the correctness of this report, and repeated, that, in 152 cases, lithotrity had been successful. M. Larrey's documents were, however, official; and the real comparative merit of the operations must still be considered as undecided.

New Surgical Instrument.—On the 2d of May, Dr. Tilhol presented to the Academy a new intrument for the purpose of making injections into the cavities of the mucous membranes, and abstracting the liquid contained in those cavities.

Temperature of the Blood.—On the 9th of May, a memoir on this subject, by M. Collard de Montigny, was read to the Academy. It maintains that the temperature varies in the course of circulation, and that (contrary to the received opinion) it is lower when the blood

leaves the left ventricle of the heart than when it enters the ventricle. M. Collard supposes the proper temperature of the to be eight degrees (?), and that the variation depends on ph causes. The novelty of the theory entitles it to mention, but we defer any more detailed account of it until after MM. Di Savart and Flourens have made their report upon it.

Use of Gold in cases of Syphilis.—On the 16th of May, M gendie made a very favourable report on a work by M. Legra this subject. The author establishes that gold acts favoural the digestive organs, without weakening the patient, and a same time produces an exhilaration of spirits. There are methods in which it may be advantageously administered; 1s tallic gold reduced to a state of extreme division; 2d. the ox gold with potash; 3d. the oxide of gold with tin; 4th. the chloride of gold and sodium. Of these the last is by far the powerful. It is applied by mixing three parts of the perch of gold and sodium with nine parts of any inert powder, and nistered by way of friction on the tongue, in doses, varying a ing to circumstances, from $\frac{1}{30}$ to $\frac{1}{3}$ of a grain per day. As m a grain has been given with safety, but this requires care. T the least expensive of all the preparations of gold. Next to strength is the oxide precipitated by tin, then the oxide preci by potash, and, lastly, the gold in a state of division, which mildest, and, at the same time, the surest form under whic administered. It is obtained by dissolving one part of percl of gold in fifteen parts of distilled water, and then pouring little by little a solution of four parts of proto-sulphate of i sixteen parts of distilled water, until there is no longe precipitate produced. The precipitates are then collecte preserved for use. This is administered by friction on the ton doses from one quarter of a grain to four grains per day. also be administered internally in a spoonful of conserve kind. The oxides are employed in the same manner, but in of $\frac{1}{10}$ of a grain to one and a half or two grains per day. T more frequently given internally, either in pills of six gr oxide, with sixty grains of extract of mezereon, or any extract of a milder character, divided into sixty pills, of whic one to ten are taken fasting in a gradually increasing ratic lozenges made of six grains of the oxides, with one ounce of po white sugar, divided into sixty tablets, to be taken in th manner. The work, which makes a tolerably thick octavo contains very copious illustrations of the subject, and also danger of the use of mercury, of which the examples are and well reported. M. Magendie, in conclusion, bestowe praise on the assiduity and research of M. Legrand, and cor that he had established the beneficial nature of his remedy, a

in administering it great attention must be paid to regulate the doses according to the strength and constitution of the patient. The work has been published some years, but has only lately attracted the attention of the Academy.

New Instrument for Lithotrity.—On the 16th of May, M. Leroy d'Etolies, to whom we are indebted for most of the instruments used in lithotrity, presented a new curved instrument, which he uses to break the stone, in cases in which a straight cylinder cannot possibly be introduced. The Monthyon prize of six thousand francs was adjudged to M. Leroy for his various instruments.

Anatomical Phenomenon.—M. Combetti, in the sitting of the 23d of May, read a memoir, containing the particulars of the case of a young girl, aged ten, who had recently died in the hospital for Orphans. Alexandrine Labrosse was born at Versailles in 1821; her father was healthy, but her mother weak, and worn out by excesses of every description. The child came into the world meagre, but well formed; it was very weak, and at two years old had not cut its first teeth. It was not able to articulate a word until after it was three years old, and could not stand alone until it had completed its fifth year. To this backwardness of corporeal developement was added a great imbecility of mind. At nine years old she was admitted into the Orphan hospital, at which time she was labouring under a paralysis of the abdominal extremities. M. Combetti did not see her until January, 1831, when she had been three months in bed. Her face was pale, and her features emaciated and oppressed with stupor; she never spoke, and, when addressed, replied in monosyllables, but always to the purpose; she lay constantly on her back, keeping her head inclined towards the left side; she could scarcely move her legs, but they retained all their sensibility; her hands were unaffected. She had long had glandular swellings of the neck; she afterwards had a mild carbuncle on the buttock, and an ulceration of the foot. She was ultimately attacked by an intestinal affection, which carried her off on the 25th of March last. The body was dissected thirty hours after the death. The lungs were found *crepitans*, but full of miliary tubercles. The intestinal surfaces offered no appearance beyond what was usual in cases of similar disease. The cranium was of the ordinary thickness; the meninges offered nothing particular, the brain appeared in its proper state, except that it was rather large. A small sanguineous effusion of recent date was observed in the thickness of the left posterior lobe. The tentorium of the cerebellum being opened, the marrow cut in the direction of the occipital orifice and the encephalic mass removed and turned over, there was observed—1. a large quantity of serosity filling the occipital fossa; 2. in the place of the cerebellum a cellular, gelatinous, semi-circular membrane of about an inch and three quarters diameter transversely, and connected with the *medulla oblongata* by

two gelatinous processes. Near these peduncles were two small v
isolated masses about the size of a pea, upon one of which was o
the nerves of the fourth pair; the quadrigeminal tubercles were u
jured; 3. no appearance of the fourth ventricle; 4. the pons va
entirely wanting, without any appearance of deperdition of substa
the anterior pyramids terminated forkwise by the cerebral pedu
It appears that the unhappy child had, from her earliest inf
contracted habits of the most vicious self-indulgence; and M. C
betti, in arguing on the foregoing facts, is disposed to attribute
absence of the cerebellum and pons varolii to a gradual destru
of those parts from disease, and not to any inherent defect of c
nization. At any rate the case presents the extraordinary
of the child having lived for some time in the possession of a
faculties, and even of a certain degree of intellect, though dep
of cerebellum, posterior peduncle, and cerebral protuber
MM. Geoffroy St. Hilaire, Blainville, Magendie, Flourens
Serres have been appointed to examine and report on the case.

Preservative against Small-pox and Measles.—On the 13th of
a letter was read from M. Remy, a physician at Chatillon, deta
some experiments which he had recently made on chloride of lin
a preservative against the small-pox. During the last autum
had observed that, out of some hundred individuals whom he
vaccinated, nearly five-sixths had not taken the infection prop
whereas in the spring, although he had used matter prec
similar, every case succeeded. He then recollected, that durin
autumn he had constantly carried in his waistcoat pocket a
packet of chloride of lime, and felt convinced that the non-rece
of the virus must have been occasioned by that circumstance.
therefore, in a village where the small-pox was raging with
violence that there only remained twelve individuals subject to t
fection who had not been attacked, caused those twelve to be wa
twice a week with a solution of chloride of lime, and gave
at the same time two drops of the solution in a glass of *eau su*
Two of them had a slight eruption similar to a vaccine, whic
not taken well; the other ten remained constantly with pa
suffering from the small-pox, without the least symptoms of il
In another village where the small-pox also raged, there
fifteen individuals liable to take it; ten of them were subjecte
similar treatment, and wholly escaped the malady; two of the
five caught the complaint. The same treatment was tried o
individuals under the influence of the small-pox; the result w
increase of inflammatory symptoms, which were removed by
ing; the progress of the eruption appeared arrested, the pu
remained in the same state as when first washed with the sol
and then dried away very slowly. This letter was referred to
Magendie and Serres. On the 20th of June, M. Chevalier rem
the Academy that he was the first who had suggested the u

chloride of lime as a preservative against the small-pox, long before the experiments of M. Remy. He also stated that the chloride of lime might likewise be used as a protection against the measles, by keeping in the chamber of the child whom it was desired to preserve from infection a saucer of dry chloride of lime, renewed from time to time, and dipping its shirts in a solution of one ounce of concentrated liquid chloride in twelve quarts of water.

Cholera Morbus.—On the 20th of June a letter was read from Dr. Foy, an eminent physician at Warsaw, the contents of which tend to prove that whatever may be the contagious properties of this disorder, their effect mainly depends upon the predisposition produced by the constitution and habits of those exposed to their influence. Habitual intemperance, disorderly living, and want of cleanliness, will generally expose those addicted to them to the immediate attacks of the disease, while the contrary habits will almost invariably be found preservatives. Dr. Foy imagines that the cholera has its seat in the spinal nervous system, and that all the functions of the skin being impeded, their restoration to their natural activity is indispensable to a cure; hence in the Russian soldiers, whose habits are disgustingly dirty, and whose skin Dr. Foy tells us was, in many instances, covered with filth of more than a twelfth of an inch in thickness, the disease generally terminated fatally. Dr. Foy exposed himself in every manner to the infection; he infused into his own veins the blood of an individual at the point of death from cholera; inhaled the breath of patients suffering under the disease; and even tasted the matter ejected from their stomachs, without sustaining any injury from the experiment beyond a slight nausea and head-ache. Dr. Foy's letter was accompanied by official certificates, affording guarantees of his experience and credibility. He also stated that the use of the tincture of *nux vomica* had been unsuccessful, but that some practioners had lately used, with good effect, chloride diluted with water.

Lacteal Infection.—The same day M. Guyon communicated to the Academy the death of an infant and a dog, who had partaken of the milk of a woman suffering from fever. The former died in thirty hours, and the latter in less than four, exhibiting all the usual symptoms of death from poison.

Zoology.

History of Zoology.—On the 26th of March, M. Geoffroy St. Hilaire read a memoir entitled 'Du degré d'influence du monde ambiant pour modifier les formes animales composant le caractère philosophique des faits différentiels.' It is impossible to follow the learned professor through his discursive, argumentative theories; it will be sufficient to state his general views, which always possess novelty and ingenuity, and often valuable truth. He commences

by distinguishing the following epochs of the science of logy:—In the first, man regarded animals merely as he wa duced to seek or avoid them; in the second, he was led by cur to examine their different forms, independently of their a utility; in the third, he felt the necessity of characteristic sign marks of distinction. The fourth was occupied by the deta nomenclature, description, and classification. In the fifth, zo attained the rank of a science, properly so called; and the e philosophical inquiry into the natural relation between the va classes commenced. During the sixth, the idea of a primitive tity of organization began to develope itself, though in a some vague and uncertain manner; and in the seventh, at which w now arrived, science is occupied in developing the external cau which originate the various modifications which that primitive tity of organization has undergone in various animals. He rem that in order to appreciate the action of external circumstances an organized being, it is not sufficient to consider that being perfect state of developement, we must study it at different peri its life. Thus we could not find any good reason for the depr and semi-elliptical form of the head of the frog, if we confine inquiries to the full-grown animal; but in tracing its origi find that the frog, in its tadpole state, partook of the orga tion of fishes, that is to say, it breathed through the volum gills placed under the back cranium. Now, the bones o auricular region are the parts covering the gills, so that their lopement must necessarily be in proportion to the volur those gills; thus the disposition of the bones of the head c frog has relation to the aquatic respiration of the tadpole. M Hilaire then traces the changes of organization through the va phenomena of the universe, showing how the organic developem animals may have been affected, at various periods of their exis by the external changes in the material world. He particularly on the changes which may have been produced in the orga respiration, and the organs dependent on them, by the ch which have gradually taken place in the atmosphere and ten ture since the primitive ages; by which he endeavours to accou the modifications which, from a comparison of fossil remain existing animals, appear to have taken place in various spec the animal kingdom. It is certain, he says, that the atmos is no longer what it was, either in its chemical or physical p ties; and as the atmosphere cannot be modified without the re tion, and consequently the whole animal economy, being mo also, it follows that the existing animals, though descending d by way of generation from the ante-diluvian animals, differ mat from them in organization. Man undergoes a change som similar; it often happens that, in consequence of a germ being in circumstances different from those in which it ought natur be placed, the being to which it gives birth does not resem

father; we then call it a monster, but all the individuals which we consider normal, would be monsters if considered in reference to their original progenitors.

Lusus Naturæ.—On the 18th of April, a communication from M. Leon Dufour, Corresponding Member of the Institut, was read to the Academy. It contained a curious account of an anomalous growth of hair in the region of the sacrum of a young man whom he had recently had occasion to examine for the conscription. This mass of hair perfectly resembled that of the head, both in length, colour, and quality. The skin from which it sprung was as white as the surrounding parts, thus preventing the possibility of the phenomenon being referred to the class of defects known by the name of moles, in which the colour of the skin is always dark, and the hair coarse and short. M. Dufour characterises the case as falling under that class of exceptions to the usual laws of organization which are designated as rudiments, and considers it as presenting the character common to several *mammiferæ*, of having the lower extremity of the vertebral column covered with long hair. The young man in question did not present any extraordinary developement of the vertebræ of the coccyx, and may, therefore, be considered as complete an anomaly as the woman with four breasts and a cow's tail, mentioned by Voltaire in his 'Philosophical Dictionary.'

On the 23d of May, M. Fabre handed to the Academy a fœtus, which had come to its full term, and even lived a quarter of an hour, having but one eye placed in the centre of the brow, and appearing to result from the junction of two eyes closely united. There was no external appearance of nose.

Fossil Remains.—At the meetings of the Academy, on the 2nd and 9th of May, M. Geoffroy St. Hilaire communicated various particulars relative to some fossil remains discovered at Caen, which belong to an animal named by him the *teleo-saurus*. He exhibited several drawings of these fragments, and also the ventral and dorsal carapaces of the animal: the former differs from that of the crocodile, which has no bony scales, whereas that of the fossil animal is composed of strong bony pieces, while a plate, equally hard, and of proportional dimensions, is under the throat, having, however, two sloping cuts, to admit of the lateral movement of the head; the latter is composed of bony scales, placed over each other, nearly in the same manner as those of the crocodile. From the peculiar organization of these animals, the learned professor concludes that they could never have breathed in an atmosphere similar to that in which we now live, but must have existed at a period anterior to the crocodiles and other animals of that species. The *teleo-saurus* must necessarily have been a marine animal, and must be referred to the same period as the ichthyosauri, the gryphites, the nautili, and other

molluscæ, the remains of which form a part of the marine dep known by the name of *terrain secondaire*, or Jurassic format No feet of this animal have ever been found; but in the cabir Caen there is a block containing the imprint of the whole ske of a *steneo-saurus*, in which is observed the form of the first joi the hind feet, which resembles that of the *ikan dugung*. It ap that there was but one middle toe, of a length beyond all propo accompanied by the rudiments of a lateral joint, thus, in som spects, resembling the horse, but as well adapted for swimmi the horse's hoof is for walking. M. St. Hilaire, however, thin probable that the feet of the *teleo-saurus* were different from tho the *steneo-saurus*, inasmuch as there certainly existed great differ in the other parts of the organization. Thus the nostrils of former are entirely terminal, giving the idea that the muzzle te nated in a sort of snout, while those of the latter are open a top, nearly in the same manner as those of the gavials. The also of the *steneo-saurus* resemble those of the gavial, while of the *teleo-saurus* are thin, and spring laterally; hence it ma supposed that the former preyed on living animals, while the lived on submarine vegetables and algæ: indeed, from the gr stones found in the midst of the fossil bones, M. St. Hilaire inclined to believe that the animal swallowed stones, for the pu of bruising and facilitating the digestion of the herbs and gr From these examinations M. St. Hilaire deduces a theory, that have been three epochs of animal creation. In the first, (to v belongs the *teleo-saurus*,) animals, without lungs, existed alone the second, animals with pulmonary organs began to appear; in the third, which comprehends the present world, the earth covered with animals of a species of which no analogous foss mains have been discovered. It results from this theory, that is of a very modern origin as compared with the age of the gl

Bicephalous Lizard.—On the 9th of May, M. Beltrami com nicated some curious particulars relative to the two-headed lizard tioned in our last Number (page 570), which lived five months i possession of M. Rigal, an apothecary at Argelles. It used its heads simultaneously for eating when it could seize its food liked. If a single insect were presented to it, both heads atten to seize it, and the one which failed endeavoured to snatch it the other. When, however, one head was satiated, the other re food, but if water were offered, the head which had not eaten v drink for the other, which then, in its turn, refused to drink wh companion was satisfied. The animal has five feet, four of v placed in the usual position, served it for locomotion; the f situated at the point of junction of the two necks, at the uppe of the common body. It has nine distinct toes, evidently res from the union of the two fore-feet. This foot, or paw, servec clean itself, and to carry the food alternately to the two mouths

it was remarked that it never presented food to the same head twice in succession, and if it had commenced with the right hand one, it invariably finished with the left. The two heads and necks are of equal dimensions, and perfectly well formed. M. Rigal had endeavoured to preserve the animal from the cold of the winter before last, by keeping it in bed during the night, and found it one morning smothered to death. It has been preserved in spirits of wine, and deposited with the Secretary of the Academy.

Collection of Natural History.—At the same meeting, M. Cuvier mentioned in terms of high eulogium the collection brought from India by M. Delamare-Picot, which he characterized as the most extensive ever made by an individual unaided by funds from government. In the zoological department it comprises 53 species of mammiferæ, 123 of fishes, 52 of crustacea, 150 of insects, 40 of zoophytes, 30 of reptiles, and 75 of birds; there were more than 400 of vegetables. Many of these species were hitherto unknown, and others were wanting in the Museum of the Jardin des Plantes, particularly the rhinoceros without horns, known by the name of the rhinoceros of Java. The mode adopted by M. Picot for the preservation and transport of his vegetables is worthy of observation. After having dried the plants in the ordinary manner, instead of placing them between sheets of paper, he put them all, pressed one immediately over the other, into flat shallow boxes, the interior of which was covered with oil of petroleum, and which were supplied with camphor and pepper pounded together, and carefully closed in all the joints. The vegetables, so packed, were not injured, either by the damp or by insects, under circumstances in which ordinary herbaries were completely destroyed. The adoption of this plan would save botanists a great deal of trouble and anxiety, and relieve them from the masses of paper which they are now obliged to carry.

On the distinguishing Marks of Venomous Serpents.—On the 16th May, M. Cuvier read a report on a very important memoir by M. Duvernoy, Professor of Natural History at Strasburg, the object of which is to point out the means of distinguishing those serpents whose bite is rendered dangerous from the venom which they instil into the wound, from those whose bite is accompanied by no evil consequences beyond those of the wound itself. The attention of naturalists has long been directed to this subject in vain. It was formerly supposed that the existence of plates or scales on the top of the head was a sufficient criterion; but a further acquaintance with the reptile tribe has proved that the rattlesnakes, the trigonocephalus, the nain, all of which are decidedly venomous, are furnished with these scales, as well as the most harmless snakes. It was afterwards thought that the jaw, remarkably moveable, and furnished with a large hollow fang, was a sign easy to be recognized, and, in fact, all serpents in which that peculiarity is observed are venomous,

but it has been discovered, some years since, that there are serp the jaw of which has not that moveable character, and contai many teeth as the common snake, but which has in front a fan easily perceived, but hollow, and instilling venom. But ever was not sufficient, as MM. Leschenault, Delalande, and Boyé a tained that some serpents, which certainly had no hollow fang front of the jaws, were unquestionably of a venomous nature; it therefore became necessary to seek in some other part o mouth the source of the poison. Accordingly, MM. de Beau Reinward, Boyé, and Cuvier ascertained that the serpent question have, in the back part of the jaw, some teeth whic longer and stronger than the others, and are sometimes hollow a manner which may be supposed as well adapted to convey p into wounds as the hollow fang of the viper. The important to ascertain, therefore, was whether these back teeth were, in connected with glands of a venomous character or not. M. Schl in a memoir printed in 1828, in the 14th volume of the 'Memoi the Académie des Curieux de la Nature,' had commenced the i tigation, and pointed out the particular glands to which hollowed back teeth can serve as conducting canals, and v glands may be co-existent with the ordinary salivary glands, a particularly noticed in the *homalopsis monilis*. M. Duvernoy, was not acquainted with the memoir of M. Schlegel, has c his investigations to a much greater extent, and has given better account than previously existed of the venomous and sal glands, and such parts of osteology and myology as relate to and which he has illustrated with carefully executed plates. observations have been principally directed to the following spe non-venomous, the *tortrix scytale*, the *coluber natrix*, the *co quincunciatus*, the *elaps lemniscatus*, the *vipera verus*, the *nai pudians*, and the *crotalus durissus*; venomous, with num maxillary teeth, the *baugarus fasciatus* and the *pelanus bicolor* finally, among those suspected of venomous properties, on ac of the long back teeth, the *coluber plumbeus*, the *dipsas interr* and the *homalopsis pantherinus*. In describing, in a very p manner, the general and particular characters of the organs glutition and insalivation, M. Duvernoy had added to, and rec the previous observations of M. Dugez, particularly with respect adductor muscle of the jaws, which he considers to be a disme ment of the mylohyoidean as well as the mylovaginian of M. D which is attached to the skin above the large scales of the jaw. M. Duvernoy has also entered into minute inquiries as proportions of the lachrymal gland, and the variation of its p within and without the orbit in different genera and species, an as to the analogy between the developement of that gland an salivary and venomous glands, and the size of the eye, a which had been left untouched, even in M. Cloquet's work lachrymatory organs of serpents. There are also severa

details on the variations in the size and developement of the sub-maxillary or common salivary gland, depending on the existence or non-existence of a venomous gland. All M. Schlegel's observations on the difference and co-existence of the two glands had been previously noticed by M. Duvernoy, who adds to them several new remarks, particularly respecting the muscle of the venomous gland, which appears to be an external temporal muscle, generally attached to the envelope of the gland, and descending to the lower jaw, without being attached to the top of the temporal *fossa*, but occasionally, as in the *naia* and the *bougares*, composed of two distinct portions. His most particular attention, however, was directed to the serpents having the long back teeth, for the purpose of ascertaining in which of them there exists the venomous gland, and in which this elongation of the teeth does not denote any specific secretion. When this gland does exist, it is frequently joined to the sub-maxillary gland by a very thick cellular tissue, and may, therefore, be easily confounded with it. The existence of this gland is certain in the *coluber Esculapii* of Linnæus, in the *coluber cerberus* of Dandin and Cuvier, the *homalopsis pantherinus* of Boyé, and in a *dipsas*, the *baugarus interruptus* of Oppel; all those, therefore, are venomous, and illustrate the observations of M. Boyé, who had ascertained, from experiments made while the reptiles were living, that the *dipsas* and the *homalopsis* are venomous. The genera *dendrophis*, *dryenus*, and *xenodon* have also the back teeth large, and even in the *dryenus nascetus*, the largest tooth is hollowed like a canal; but as M. Duvernoy has not found any specific or venomous gland, he concludes that they are not poisonous. These circumstances explain the contradictory testimony existing respecting the venomous qualities of particular serpents, and at the same time prove that the class in question must be far less dangerous than those in which the fang conducting the poison is in front, because unless the object bitten be sufficiently small to admit of its being taken into the mouth of the serpent, and thus brought into contact with the back fang, no poison will be communicated to it, and only a common wound be produced; so that a person bitten in the leg or arm would suffer no injury beyond the actual bite, while another, whose finger was inserted into the mouth of the reptile, would be poisoned. Hence, M. Duvernoy concludes that the principal use of these posterior fangs is to kill the small animals which the serpents take into their mouths alive, and that they are not of much advantage as a means of attack or defence against external enemies. In the course of his Memoir, M. Duvernoy remarks, that in many serpents the spleen is closely attached to the pancreas, which probably led M. Meckel into the error of doubting its existence. This memoir was ordered to be inserted in the 'Recueil des Savans Etrangers,' and the various preparations presented to illustrate the subject were deposited in the Gallery of Anatomy in the Museum of Natural History.

Organic Symmetry.—On the 23rd of May, M. Dutrochet, a responding Member of the Institute, addressed a letter to Academy, relative to the want of symmetry observed in the int organs of a great number of animals arrived at their fullest of developement. He does not agree with Bichat, in supposing want of symmetry to be an essential character of the organs; on the contrary, agrees with M. Cuvier, that in animals with bodies there is an evident symmetry existing, which is still striking in the fœtus during the first periods of its existence alimentary canal is then extended in a right line from the n to the anus, and is perfectly symmetrical. In the original of organization, the symmetry was as perfect internally as externally; and if it be afterwards destroyed, it is by a sp of abortion of one of the sides. In the larva of the ac salamander, when it first leaves the egg, the alimentary ca perfectly symmetrical. On its two sides, near the beginning c intestine, are perceived the liver on the right, and the spleen c left, forming almost a perfect symmetry, as there is scarcely a ceptible difference of size, and the form, as well as the positi precisely similar. In process of time, however, the *left liv* spleen, becomes an abortion, and is consequently without funct so also in the primitive organization of insects, the biliary orga symmetrical. Sometimes two symmetrical organs will both b abortions: they will then be useless to the body, serving o indications of the primitive organization. Such M. Dutroche poses to be the history of the *capsulæ renales*.

Classification of Lusus Naturæ.—On the 11th of April, M. G St. Hilaire communicated the substance of a memoir which l prepared, on the classification of a particular family of lusus n which he considers as forming a regular series of anomalous l the fundamental character of which depends on the union upper part of the nervous cerebro-spinal system of two indiv in a single system, which is either doubled by the fusion c complete systems, or single by the combination of two sponding halves. The encephalus is to be considered as cor of four systems of lobes—the spinal marrow, the cerebellu optic or quadrijumal lobes, and the cerebral lobes. In the in question, the ventral regions remain distinct; the two bein perfectly separate, and subject to the ordinary rules of or tion below the navel, but above it they are united and confo The vertebral columns inclined forwards unite beyond th and each produces half of the cephalic elements which te them. The following are the names and characteristics of t classes into which M. St. Hilaire divides this family:—

1. *Deradelphus*—Cephalic elements double as far as resp medulla oblongata and the occipital part of the brain *(hype de l'occipital)*. The rest of the head single.

2. *Synotus.*—Cephalic elements double as to the medulla oblongata and cerebellum; the rest of the head single. Ears supernumerary, and united behind the head.

3. *Eniops.*—Cephalic elements double as to the medulla oblongata, cerebellum, and optic lobes; rest of the head single. Supernumerary ears behind the head, and an additional eye in the sinciput.

4. *Janiceps.*—The whole encephalus and organs of senses double; the faces opposite each other.

Examples exist of each of these classes, but the first has only recently been met with. The name given by M. St. Hilaire to the family is that of "Monstres bicorps unicephales."

Miscellaneous.

Chronology of the Egyptians.—A considerable portion of the sittings of the 4th and 11th of April was taken up by the communication of a memoir by MM. Biot and Champollion on this interesting subject. It is well known that the Egyptians divided the year into twelve months of thirty days each—which, with five intercalary or supplementary days, completed the number of 365. Twelve great divinities presided over the twelve months of the year, five others over the five intercalary days; thirty genii regulated the thirty days of the month; and the twenty-four hours of the astronomic day were under the protection of twelve gods and the same number of goddesses. This year of 365 days was, however, about a quarter of a day shorter than the solar year, whence the first day of the month of Thot, which began the year, was perpetually in advance of the sun's progress in the ecliptic: so that if the 1st of the month of Thot occurred at the vernal equinox, it would, in four years, be one day before it; and so on in progression until the expiration of 1506 years, when it would again occur at the precise period of the equinox. Hence the Egyptian year was termed *annus vagus;* and so great was the attachment of the country to it, that the kings, on coming to the throne, were compelled to take an oath against allowing any change to be made in the mode of computing the year; and the compulsory correction made in the calendar by Augustus, twenty-four years before Christ, was considered one of the bitterest fruits of the Roman conquest. This attachment was not founded on ignorance; on the contrary, the Egyptians were well aware that the solar year was about a quarter of a day longer than their *annus vagus*, and were probably even the first to communicate that fact to the Greeks. This strong attachment to the *annus vagus* is the more remarkable, when we consider that none of their monuments give us any reason to suppose that these years were connected by them in any regular chronological series; on the contrary, all the Egyptian dates which have reached us are reckoned

from the commencement of the reign of each king: so t
order to establish the historical succession of events, it woul
rently be necessary to have a chronological canon, indicati
number of years of each reign; and many antiquarians ha
posed that this was the case. It is, however, singular, that
numerous Egyptian monuments with which we are acquainted
no traces of any such canon—except, indeed, the chronologica
of Ptolemy, and the fragments of the chronicle of Manetho,
which are of very limited extent. M. Biot seeks to prove, t
attachment of the Egyptians to the *annus vagus* arose from
of its containing a natural cycle, specially adapted to their c
so that, by means of symbolic signs attached to the different
the *annus vagus* and to certain epochs of the true solar ye
could, with the utmost facility, connect these two systems o
and thus fix the dates indicated by the *anni vagi*, with as m
cision as we can do by our present calendar. M. Champo
his late researches, has ascertained that the Egyptians div
year into three equal portions of four months, or 120 days e
five supplementary days having a separate and distinguishing
these were represented by symbols illustrative of the periods
tation, harvest, and inundation. The inundation of the N
mences invariably at the summer solstice: it attains its
elevation in 100 days, and then, after remaining stationa
days, it begins to recede, and the ground is sown while ye
so that in 120 or 125 days after the summer solstice, the
inundation ends, and that of vegetation commences. Fou
afterwards the harvest begins, and lasts four months; a
ends the agricultural year. The Egyptians were well aw
as the inundation of the Nile invariably commenced at the
solstice, which would therefore be the first day of the nint
or third period of the year, the first day of Thot (the firs
ought to be 125 days after that solstice. They had, there
to observe the degree of variation existing between the tin
solar year at which the 1st of Thot occurred in any given
that at which it ought in reality to be found, and a perp
unfailing calendar was at once constituted, showing with t
precision in what part of the cycle of 1506 years any g
was. It only remained then to ascertain how many of th
there had been, or, in other words, when the Egyptians firs
the mode of computation by the *annus vagus*, and ob
variation from the true solar year. It is evident that t
must have commenced at a period when the solar yea
annus vagus were in accordance; and as we know t
Augustus altered the mode of computing the year, t
years B.C., the 1st Thot of the *annus vagus* corresponde
29th of August, it is easy, by fixing the date of the summ
to ascertain when the 1st day of Thot did in fact occu
after the summer solstice. The following table show

periods before the Christian era at which that coincidence took place:—

Julian era.	Years before Christ.	Date of 1st Thot.	Date of Summer Solstice.
76	4790	December 4	August 1
1429	3285	November 22	July 20
2934	1780	November 11	July 9
4439	275	October 31	June 27

It is therefore to one of these four periods that the commencement of the Egyptian mode of calculation must be attributed. The last of the four may be excluded from our consideration, because the researches of M. Champollion have proved incontestably that the *annus vagus* of 365 days was used by the Egyptians previous to the year 1600 before Christ. We have therefore only to examine the probability attaching to the first three; and for this purpose it is important to remark that the Egyptians, in their mythological system, considered the star Sirius as the power influencing the rising of the waters of the Nile; and therefore it must be presumed that their system commenced at a period when the heliacal rising of Sirius coincided with the summer solstice, or rising of the Nile. This occurred for the first time in the year 3285 B.C., previous to which the rising of Sirius was more and more removed from the summer solstice; the date of 4790 B.C. may therefore be also put out of the question, and the doubt only remains between the years 3285 and 1780 B.C. The first, as we have before seen, coincides precisely, and therefore furnishes a fair presumption that the Egyptians at that period formed their year of 365 days; but we cannot be quite certain of the facts, because, supposing the addition of the five days not to have been made until 1780, and consequently calculating backwards by years of 360 days, we should only make a difference of six Julian years, placing the period of coincidence between the summer solstice and the 1st day of Paschous (the ninth month) in the year 3291, when the heliacal rising of Sirius differed only a day and a half from the summer solstice—an error which may easily be committed in determining the heliacal rising of a star from observation only.

From these observations, we arrive at the following important conclusions:—1st. That the Egyptians, knowing that the cycle of variation between the solar year and the *annus vagus* consisted of 1506 years, could always tell, by observing the number of days which the 1st of the 9th month (Paschous) varied from the summer solstice, in what year of the cycle they were; and that at the time of the alteration of the year by Augustus they could not be in more than the third, and perhaps only in the second of such cycles from the time of their first calculating in that manner. 2nd. That as all the data on which this calculation of the year is founded relate to the phases of the Nile, it is evident that it is of Egyptian, and not of Chaldaic origin. 3rd. That as the phenomena relating to the Nile continue at the present moment to occur in precisely the same manner and at the same intervals as at the commencement of the

Egyptian calculations, it is evident, that for 5000 years the d
bution of the terrestrial heat on the surface of the earth has rem
the same, as any change must have affected the periodical rai
Upper Ethiopia, the rising of the Nile, the duration of the inu
tion, &c.

Protection of Firemen.—On the 4th of April, M. Gregori
municated some details of the experiments recently made in
by the Marquess Origo, the commandant of the firemen at R
with a view to guarantee them from the effects of entering h
while a prey to conflagration. Acting on the received opinio
the Romans employed a mixture of clay and vinegar to extin
flames, he tried that mixture in every manner, but it produc
satisfactory result. He then dipped two complete suits of fire
dresses, including boots, gloves, and two cowls, made of the
cloth as the dresses, in a solution of sulphate of alumine and su
of lime, and, when dry, saturated them with soap water.
firemen were clothed in these dresses, and their faces covere
incombustible masks, covered with cloth saturated with a
solution; the openings for the eyes were covered with a v
amianthus, and small damp sponges were placed in their mou
ears. Thus protected, they entered a house, 23 feet long
feet wide, filled with burning wood, which they traversed ten
without the slightest injury. Their clothes were not da
although they had remained fifteen minutes exposed to the
of the flames. The only effect produced on the men was the i
of the pulsation from 70 to 125. These dresses cost but two
sterling each; and are, therefore, in that respect, more eligib
those composed of amianthus, as recommended by the C
Aldini. M. Origo also extinguished flames of considerable
by playing on them with the solution of sulphate of alumi
clay, by means of a common engine.

Transport of Edifices.—On the 9th of May, M. Gregori
to a circumstance mentioned in a late Number of the 'Jou
Artistes,' of a rock of granite, 42 feet long and 27 high
been transported from the bay of Finland to St. Petersburgh,
as a pedestal to a statue of Peter the Great. He state
much more remarkable fact had occurred at Crescentino
when a common mason, named Serra, succeeded in trans
brick belfry (which he had contrived to cut from its base
injuring the walls) from one church to another, at a con
distance. While it was being moved, a man inside rang
A model of the machine, employed in the transport, was
in the library of the Institute.

Travelling in India.—At the same meeting, M. Elie
mont read extracts from two letters, which he had received
Victor Jacqueminot, a French naturalist, travelling in In

Jacqueminot censures the name of Valley of Dhoon, given by the English to the valley at the entrance of the Himalaya, as being a mere pleonasm, the word Dhoon signifying valley. The Valley of Dheynia is its proper name: it is a longitudinal valley, hollowed between the foot of the Himalaya, properly so called, and the raised diluvial earth. Thence he visited, on foot, the sources of the Jumna. In this expedition he passed over heights of an elevation of 5550 metres (18,208 English feet). He penetrated more than once into the Chinese territories; and, in returning towards Ladack, he slept at a village called Ghyournneul, situated on an elevation of 5000 metres: on the Indian side of the Cordilleras, he did not observe any village at a greater elevation than 2700 metres. Cultivation, also, on the south side, stops 2000 metres below the level which it attains on the Thibetian sides of the descent. This difference arises not so much from the temperature as from the state of the sky, which is cloudy and rainy on the Indian side, and pure and free from humidity on the other side of the Himalaya. From a variety of geological observations, M. Jacqueminot is induced to think that there exists a difference in the age of the Thibetian and southern chains of the Himalayan mountains; an observation which M. de Beaumont had already made relative to different chains of the Alps. M. Jacqueminot also mentions the uncertainty of correspondence in that part of the country,—as, in addition to the ordinary casualties of letters, the couriers between Benares and Calcutta are occasionally devoured by the tigers *en route*.

Paganini.—On the 16th of May, Dr. Bennati read a physiological notice of this extraordinary man, in which he gives it as his opinion, that the prodigious talent of this artist is mainly to be attributed to the peculiar conformation which enables him to bring his elbows close together, and place them one over the other, and to the elevation of his left shoulder, which is an inch higher than the right one—to the slackening of the ligaments of the wrists, and the mobility of his phalanges, which he can move in a lateral direction at pleasure. Dr. Bennati also alluded to the excessive developement of Paganini's cerebellum, as connected with the extraordinary acuteness of his organs of hearing, which enables him to hear conversations carried on in a low tone at a considerable distance. M. Geoffroy St. Hilaire remarked that he had been particularly struck with the prominence of the artist's forehead, which hangs over his deeply-seated eyes like a pent-house.

Oil Cloths.—On the 23d of May, M. Chevallier pointed out a very simple method of removing the unpleasant smell which has hitherto militated against the use of oiled or varnished cloths and stuffs. It is merely to expose them to the action of a chloric fumigation in a close room.

Prevention of Falsification of Written Instruments.—The attention of the French government has long been directed to the possibility of finding some means of preventing writing being chemically

discharged from papers and other documents, either for the pur of falsifying the contents, or for making a second and fraudulent of old stamps. With this view, the Academy of Sciences directed to take the subject into consideration; and a commi consisting of MM. Gay Lussac, Dulong, Chaptal, Deyeux, Then D'Arcet, Chevreuil, and Serullas, was appointed for the pur The attention of the public was called to the subject, and a g number of specimens of ink, alleged to be indelible, were forwa to the committee. Numerous experiments were made; and o 30th of May and 6th of June the report was read to the Acac by M. D'Arcet. It is unnecessary for our purpose to follow reporter through his elaborate history of the different manufac of ink in different ages, or the detail of the experiments made the various samples submitted to the committee: it is sufficie state the conclusions, which were unanimously adopted as results of the investigation. These were, that the falsificati written documents will be fully prevented by the use of ink pre in either of the two following manners. 1. Indian ink (or, i absence, the imitation of it made in Europe with soot and anima or gum), dissolved in a mixture of water and muriatic acid, c specific gravity of 1010 (1½ degree of Beaumé's instrument.) ink may be prepared for fourpence English per quart. 2. To a sol of acetate of manganese, of the specific gravity of 1074 (10 de of Beaumé), add half its volume of solution of carbonate of crystallized, saturating it at about 166 per cent.: dissolve India in this liquid, and writing traced with it will become perfectly lible on being exposed to the action of the vapour of liquid amn The committee lay down, as a general principle, that no ink in a liquid state, can be indelible, as the colouring matter, fr excess of density, will necessarily be deposited. Additional se will be obtained by writing on paper so prepared, that even ink could be discharged, it would necessarily be seen that been so discharged. Thus, M. Coulier proposes a paper, h printed on each sheet, lines and patterns, so complicated, as t forgery, and struck off from a steel plate damasked with aqua The ink with which this is printed would be discharged by ch so that the superjacent writing cannot be destroyed without a stroying the drawing. This plan is excellent for bills of exc and other small documents; but from the expense and dela sioned by the engraving and printing, the designs would adapted for legal proceedings and public documents. M. Ch proposes a paper coloured in the pulp with colours liable to charged by all the known re-agents, but this might easily coloured when the alteration is made. M. Maimu suggests to the pulp of the paper filaments of wool, cotton, or hemp, different colours, some of which will be acted on by the acic others by the alkalies, but all liable to be discharged by cl When these colours are discharged, it is almost impossible to them; but the writing may, in some cases, be effaced with

sensible alteration in the colour of the filaments; and on the other hand, that colour will frequently change by simple exposure to the air, without any re-action being used. Mr. Coulier's method is by far the best, but has the disadvantage, that all designs easily dischargeable from the papers may become injured by time or accidental circumstances, a consideration which, in cases of forgery, would tend to render probable the impunity of the guilty by the fear which would be entertained of condemning the innocent. The use of these prepared papers must, therefore, be considered as very secondary, the main security must be found in the indelible inks. The discharge of the writing from old stamped documents, and the consequent fraudulent use of the stamp, may be prevented, 1st. By printing on all stamped paper, by means of a cylindrical press, an engine-turned vignette, placed on the right of the stamp, in the centre and along the whole length of each sheet. 2nd. By employing, in printing these vignettes, a colour having for its base the black precipitate formed in the dyeing coppers of hatters, or ink thickened in the manner adopted in the manufactories of painted cloths; and 3rd. By marking on all stamped papers the date of their fabrication, either by printing it in the pulp, or engraving it on the vignette or the stamp; or, more simply still, by making the dry stamp, impressed on each sheet of paper, revolve, so as to affix a new date each year. This report was ordered to be transmitted to the Minister of Justice.

Gelatine.—The discovery of Mr. D'Arcet, member of the Institute, of the means of preparing the gelatinous matter of bones, so as to form a cheap and wholesome article of food, has excited great attention in Paris. More than two years have elapsed since the discovery, and the system of M. D'Arcet has been adopted in several of the hospitals, and in the *Maison de Refuge pour l'Extinction de la Mendicité* of M. de Belleyme. The gelatine has also been used in making sea-biscuits, which were used by the troops during the late expedition against Algiers. The mode of preparing both the gelatine and the biscuits is minutely laid down in the pamphlets published by M. D'Arcet. These experiments had invariably been attended with success; but on the 6th of June, M. Donné, a young medical student, communicated to the Academy some remarks tending to throw a doubt on the subject. He stated, that being deeply impressed with the importance (particularly to the lower classes) of ascertaining whether the gelatine did really possess the nutritive qualities attributed to it by M. D'Arcet, he resolved to go through a series of personal experiments on the subject. With this view, recollecting that ten *grammes* of dry gelatine were stated to be equivalent to half a litre (about two basins) of the best meat broth, he began by taking that quantity every morning with three ounces of bread, and gradually increased the quantity up to fifty grammes, which constituted his sole nourishment up to six o'clock every day;

the gelatine was differently flavoured, so as to prevent its exc any feeling of nausea or disgust. During the six days which experiment lasted, M. Donné experienced a constant sens of sinking and feebleness, and on the sixth day found that he lost two pounds weight. The next week he substituted ordi meat broth for the gelatine, taking a litre and a half (about five o bowls), and from four to five ounces of bread daily; during week he experienced no sensation of feebleness, and at the end had regained a pound and a half of his lost weight. At the s time M. Donné tried similar experiments on two dogs, giving the gelatine mixed with a little bread, and offering the other nothing simple gelatine. The former at first refused it, but at length daily as much as was equivalent to twelve or fifteen half litre good broth. On the sixth day the dog had lost four ounces in wei and was so voracious that he even greedily devoured some w lead prepared for cleaning plate, and during the second week to refused gelatine, living only on about an ounce and a half of b which was given him per day. He ultimately terminated the e riment by climbing to a great height, and taking possessio a quantity of boiled beef which was supposed to be out of his re The other dog could not be prevailed on to touch the gelatine, after being for five days totally without food. M. Donné, there considered it cruel to pursue the experiment further, and gave his usual food. From these circumstances M. Donné was ind to doubt the nutritive qualities of gelatine, and begged the Acad to appoint a committee to investigate the subject, which was acc ing done. At the succeeding meeting (13th June) M. D'Arcet dressed some observations to the Academy on the subject allude by M. Donné; he stated that butchers' meat contained, on average, in every 100 lbs.—

Dry meat . .	24 lbs.	
Water . . .	61	
Bone	15	
		— 100

Bones contain, on an average,

Earthy substance .	60 lbs.	
Gelatine	30	
Fat	10	
		— 100

From this calculation it is evident that the 15 lbs. of bone tained in every 100 lbs. of meat would furnish $\frac{40}{100}$ of their we or 6 lbs. of animal substance, so that 100 lbs. of meat, which furnish but 24 lbs of dry meat, might, by rendering the gel and fat of the bones available, supply thirty, or, in other words, oxen would furnish as much alimentary substance as is now obta from five. With respect to the nutritive and salubrious qualiti gelatine, he remarked, that the committee appointed by the Fa of Medicine, consisting of MM. Le Roux, Dubois, Pelletau,

meril, and Vauquelin, after having given gelatine soup to forty patients and others, during a period of three months, came to the conclusions:—1. That the use of gelatine was both an amelioration and a source of economy in the alimentary system. 2. That gelatine soup is at least as palatable as the ordinary hospital soup; and, 3. That gelatine is nourishing, easy of digestion, and wholesome, and cannot, in any manner, be productive of injurious effects on the animal economy. The apparatus in the hospital of St. Louis is capable of preparing nine hundred soups per day; it has been in use twenty months, and has supplied 550,800 portions of gelatinous food. Numerous reports have been made on the subject to the general administration of the hospitals, all of which agree in stating that the change in the mode of nourishment is a decided improvement; that the convalescent patients acquire strength much more rapidly than before; that it is a source of economy highly important to the poor; that part of the meat formerly employed in making soup may now be given to the patients, either roasted or in other forms, and, finally, they all recommend the adoption of the system of gelatinous nourishment in all similar establishments. At the Hotel-Dieu, 443,650 rations of gelatine have been furnished in fifteen months and a half; and six reports have been made, all of which are equally favourable with those above referred to. They state particularly that since gelatine has been employed, thirty kilogrammes of roast meat may be given to the patients daily, without reducing the quality of the soup at all below its former standard.

When M. D'Arcet had concluded his remarks, M. Gay Lussac animadverted in strong terms on the injustice and insufficiency of the mode of experiments adopted by M. Donné, which he characterised as wholly inconclusive, although calculated to produce a most injurious effect on the public mind, which is always easily impressed with the idea that the poor are neglected, particularly in hospitals. He reminded the Academy that it was well known that no single substance was alone sufficient to support animal nature; that animals fed on sugar alone had died from inanition; yet it would not be pretended that sugar is destitute of nutritive qualities; and though the nutritive qualities of potatoes, taken with other food, are universally known, a dog fed wholly on that vegetable dies in six weeks; whereas M. Donné wishes it to be supposed that because two dogs refused to live upon gelatine, administered alone, we know not how, and because M. Donné himself grew thin on a sudden adoption of simple gelatine diet, the adjunction of gelatine, as an addition to, and taken in conjunction with animal food, is wholly without advantage. On the 20th of June, M. Donné replied to M. Gay Lussac, by saying that his sole object in proposing the question was to have it fully and fairly investigated; since if it can be established that gelatine does possess the nutritive qualities ascribed to it, the advantage to the poorer classes will be immense; whereas, on the other hand, should they be induced to employ the bones as a means

of nutriment, when the fact may turn out to be that the gelati not nutritious, their condition is rendered more deplorable before. In conclusion, he said that he rendered full justice tc active and pure philanthropy of M. D'Arcet, which had induced to make the greatest sacrifices both of time and money, in ordε bring the gelatinous system to perfection.

Mirage by Suspension.—On the 20th of June, a letter was from M. Rozet, stating that he had frequently remarked this at pheric phenomenon in the neighbourhood of Algiers, and part larly on the 27th of June, 1830, when about ten o'clock in the m ing, at which time the sky was perfectly clear, and the thermon at 21° (Reaumur), he distinctly saw, when looking at the lin battle formed in the camp at Staonelli, two images of all the obj the mirage being about half as strongly marked as the real im but still perfectly distinguishable and elevated above the object a one-fourth of its height, deviating a little laterally. On the Alg tents, surmounted with tin spheres, with a crescent on the top. image of a second crescent forming a tangent to the first, was tinctly visible, so that, at first sight, it appeared as if there were crescents to each tent. When the images are reversed, they rarely clear, and have always a perceptible movement of undula

Climate of Algiers.—The same letter stated, that whenever south wind blows in the neighbourhood of Algiers, the temperatu raised from 5° to 10° C. (41° to 50° F.) On the 17th of Septembe thermometer stood at 39° (103° F.) in the shade. Those who happ to be affected by drinking at that time, suffered severely, falling d insensible. This wind rarely lasts twenty-four hours, and occas as much inconvenience to the natives as to the French. Storm not frequent at Algiers, but when they occur they are of violence. On the 8th of May last, the whole horizon was a she flame; a strong white light rested for half an hour on the extren of the flag-staffs of the forts of Algiers and its vicinity, and the offi who were walking bareheaded on the terrace, felt their hair stan end, and perceived a luminous star at the extremity of each same species of star was observed on the ends of the fir when held upright in the air, but disappeared when held downw During these storms every one is affected with great lassitude, p cularly in the legs, and experiences strong nervous agitation.

New Chart.—At the same meeting M. Coplin presented a graphical chart of the islands of Perouse, in which, by a new pl drawing, in imitation of relief, he has succeeded in so well av himself of the process of shading, that not only the geological c tution, and the direction of the declivities, but also the variatic the surface of the different mountains are distinctly exhibit the eye.

Miscellaneous Scientific Proceedings on the Continent.

ACADÉMIE DES INSCRIPTIONS ET BELLES LETTRES.

Indian Antiquities.—On the 22d April, MM. Quatremère, Lajard, and Abel-Rémusat, made a report on the antiquarian part of M. Lamare-Picquot's collection of curiosities, brought from Hindostan. The zoological part of this splendid collection has formed the subject of a report to the Académie des Sciences (vide p. 157), and the present reporters had, therefore, only to occupy themselves with such parts as tended to throw a light on the civil and religious manners and customs of the Hindoos. The greater part of the articles relating to the Brahmin religion are from Calcutta and its neighbourhood; those which relate to the worship of Buddha are originally from the Burmese empire, whence they were taken during the war with the English in 1825; and a few remarkable curiosities are from the isles of the Ganges. There are about fifty figures representing the divinities of the Brahmins; these are in terra cotta, marble and bronze, and present images (some of them in bas-relief) of Brahma, Vishnou, Sheva and his wife, Parvati, Krishna and his wife Radha, Gamessa, Balarama or Vishnou as a child, Jaghernout, Dharma-Deva, or the god of the law, under the figure of an ox; Dourga the wife of Sheva; Kali, the same goddess with the attributes of goddess of death, those of protectress of the universe, and those of her combat with Mahichaasoura, the genius of evil, under the form of a buffalo. There are also several mythological subjects, executed on pasteboard, by Hindoo painters; and a large picture representing the combat of Rama against Ravana, the tyrant of the isle of Lauka, a subject taken from the Ramayana or the Baghavata-Pourana. M. Lamare-Picquot has also brought over a number of vases, lamps, and other religious and domestic vessels of the Hindoos. He has also succeeded in procuring three or four Berchocath, or pieces of carved wood, representing towers with several stories, enriched with a variety of paintings and ornaments. These are carried in the funeral processions of the Hindoos, and then placed near a pagoda on the banks of the Ganges, or some other consecrated river. The collection also includes a variety of exact models of the Hindoo temples, and a sort of fetish, found in an island of the Ganges, representing a head surmounted by a rudely shaped mitre, and coloured equally coarsely. The reporters have not discovered who is represented by this figure. The figures relating to the worship of Buddha are fewer in number, but of considerable dimensions. There are thirty statues of Gaouatama in terra cotta, wood, copper, marble, and alabaster, all exhibiting traces of gilding, and varying in size from one to three feet. This personage is always represented in the act of divination, in a sitting posture at the moment of inspiration, the head surmounted with the characteristic tubercle, the hair in ringlets, half naked, and the right hand pendant. Two only of these statues have inscriptions, one of

which is in Burmese, and the other in Bengalese. Smaller f in bronze and lead represent other and secondary divinities. the rarest pieces is a small group representing eight diviniti sent at the birth of a Shakia. There is also a fine and large ba in terra cotta, of Burmese workmanship, which was intende placed over the entrance of a temple; it represents two lions, red, in an attitude of repose, and separated by stalks of reminding us of the celebrated religious monuments of W Asia. The objects not relating to religious ceremonies are of different classes of Hindoos in their proper costume; the are in terra cotta, and the dresses in real stuffs; many of th executed with great perfection, although the Hindoos of I gore, by whom they are done, have not practised that kind more than fifteen years. There are also a great variety of d utensils of the Hindoos calculated to throw great light habits and manners. The reporters, in conclusion, bestow highest praise on the perseyering assiduity of M. Lamare-] and strongly recommend the formation of an Ethnographic M similar to those existing at St. Petersburgh and various t Germany, for the preservation of all the objects of every calculated to throw light on the manners and customs of an of the globe.

SOCIÉTÉ D'ENCOURAGEMENT DES ARTS ET DE L'INDUST

Enamel Painting.—On the 1st of June M. Merinnée report on a new application of enamel painting, which pro be of great importance to the arts. This branch of art has been confined to painting on enamelled metallic plates, or lain: the objection to the former is that, in consequenc action of the fire on their shape, they can never be used certain and small size; while the latter, though presentin vantage of greater dimensions, has the inconvenience of susceptible of being passed above three times through because the enamel of the porcelain not having the same as the colours, the latter scale off when the action of the f longed beyond a certain point. The difficulty, therefore, w a substance which, while it afforded equal dimensions plates of porcelain, would support the action of the fi breaking or losing its form. This want has been suppl discovery of the properties of the lava which is found in g tities in the mountains of Puy-le-Dome, and to which guishing name of *tephrine* has been given: that proc Volvie is the best. This lava is very porous, and co lighter than common stone. It is sawn into plates o thickness (about half an inch); and when these plates cut perfectly even, the small cavities of the surface are with a vitrifiable paste, which, by the action of the fire, substance with the lava, and subsequently unites itself the layer of enamel which is placed over it. Plates of

four feet long are thus prepared without much trouble or expense, and they may be made double the size. The blocks sometimes taken from the quarries have a superficies of ten feet square. The enamelled surface of this lava is not even, like the enamel of porcelain; but it is a little grained, which renders it particularly adapted for pictures on a large scale, as historical pictures, &c. If it were required to use this substance for miniature painting, the layer of enamel must be perfectly smooth; and though this would be difficult to effect, the reporter is of opinion that it would not be impossible. The Count de Chabrol, when prefect of the Seine, first employed this lava for the *trottoirs*, or foot-pavements, of the streets; and M. Mortelique, being induced from its fusibility, its vitreous qualities, and its porous consistence, to suppose that it was susceptible of being enamelled, made a variety of experiments, and ultimately, in 1827, exhibited a head, painted the natural size, on a plate of this lava, which was considered worthy of a prize. But in order to render this generally useful, it was requisite to make the enamel painting so nearly analogous to oil painting, that historical painters might acquire the art without material loss of time. The great difference was that, on the enamel, as on porcelain, the colour could only be applied by small touches in juxtaposition, and could only be degraded by letting the white ground appear more or less through the transparent tints. This mode of proceeding, which is that of miniature painters, is much too tedious for artists accustomed to lay the colour thickly on the canvass. M. Montelique has therefore applied himself to the discovery of a white which will combine itself with all the colours used in enamel painting, without decomposing them. In this he has fully succeeded, and by this discovery has removed the only difficulty existing in the use of the lava for paintings; so that pictures of the largest size may now be painted in enamel with the same facility as in oil; and with every facility of retouching the picture, when in progress, is combined the advantage of the colours being rendered capable of bidding defiance to the ravages of time, by the unlimited manner in which they may be passed through the fire. Had this discovery been made three centuries earlier, we should not have to deplore the deterioration of the 'Last Supper' of Leonardo da Vinci, and the 'Descent from the Cross' of Daniel di Volterra.

Société de Géographie de Paris.

Annual Prize.—This prize was proposed for the most important geographical discovery made during the year 1829. The committee, in their report, first mentioned, in terms of praise, Captain King's attempt to explore part of Patagonia, but added, that as his voyage has not yet been published, no judgment can be formed of the importance of the results at which he has arrived. M. Parchappe, by his discoveries in South America, has thrown new light on the course of the Uraguay, and other rivers of the basin of Parana. This traveller, in the twelve years which he has passed in the province of

Buenos Ayres, and those watered by the Parana and Uragu rectified some remarkable errors, particularly that which assi the Lake Ibera, from east to west, four times its real lengt has also ascertained, in a satisfactory manner, the course o of the rivers Colorado and Negro. The committee als favourably of the voyage of circumnavigation of the Russia Moller and Seniavin, commanded by Captains Starikowi Lütke. The latter, in particular, has discovered new island archipelago of the Carolinas, particularly the island of Poun habited by a race of blacks analogous to that which peo coast of New Guinea; whereas all the islands of the arch previously known, are peopled by the copper-coloured rac forms the intermediate link between the Malays and the Pol properly so called. The prize, however, (consisting of a go of five hundred francs,) is adjudged to Captain Graah, of th navy, for his exploring voyage along the eastern coast of Gr to which he penetrated by sea, and discovered a people wh remote age, have been deprived of all communication with and whose language was nearly unintelligible to the Greenl preters who accompanied him. They retained some vesti Christian religion. The eastern coast of Greenland was very little known. Between Cape Farewell 59° 42′ lati Cape Barclay 69° lat., very few points were known; the supposed to proceed in a north-easterly direction, bu Graah has ascertained that its direction is nearly north. was discovered about the year 982, by Eric Rauda, and wegians, in the succeeding ages, sent missionaries there colony appears to have entirely dropped into oblivion fifteenth century; and though it was vaguely said that differing from the Esquimaux in habits and physiogno somewhere, it was reserved for Captain Graah to asce existence with certainty, and make their situation known Captain Graah's Journal will shortly be published, and wi throw much valuable light on the real direction and posi coast of Greenland.

ACADEMY OF SCIENCES OF ST. PETERSBURGH.

Meteorological Phenomena.—On the 9th of Febru communication was made of a singular phenomenon, Oremburg, on the 1st December. During the whole rain fell, although the thermometer remained steady point: about midnight three loud claps of thunder were north-westerly direction; the next day there was a fall companied by a multitude of little gnats, the motions o similar to those of the flea. The day after the atmosp up, and the thermometer descended ten degrees belo the same meeting a letter from the Governor of Orembu stating that on the 7th of January, between six and evening, the moon, which was nearly new, appeared

with a large and perfectly regular luminous circle, cut by two diameters equally luminous: the moon occupied the centre of the circle. Two white semicircles were distinctly traced at the extremities of the diameter, which cut the circle from east to west, and their light was reflected almost as far as the extremities of the other diameter which divided the circle with the same regularity from north to south. To the north of this circle was observed a luminous arch of small dimensions. During the whole time of this phenomenon being observed, the atmosphere was pure and tranquil, and the thermometer was not below seventeen degrees (Reaumur); a short time afterwards it fell to twenty-nine degrees below freezing point.

New Mineral.—In the month of August last, the Academy was presented with a new mineral found in some government-lands in the province of Perm. It has received the name of *Volkonskoïte*, in honour of Prince Volkonsky. The spot in which the vein was found is in the mountain called Efimiatskaïa, in the district o Okhausk. The bed does not consist of regular veins, but in bits of from one to four *verschocks* thick, by a quarter to three-quarters of an *archine* long; sometimes ten of those bits or patches are found in the space of a single *sagene*, and sometimes there are three *sagenes* without a single one. The mineral, in colour, approaches the grass-green; it divides in longitudinal plates, and breaks on the slightest pressure. When plunged in water it separates with a loud noise into angular pieces, on which, when dried, the water no longer takes any effect. This mineral may be employed as a colouring matter to replace some of the most expensive colours, such as molochite and verdigris. The fine orange colour of chrome may also be chemically obtained from it, as it contains about seven per cent. of extract of chrome. It is easily worked and at a small expense.

Mineralogical Society of Russia.

Native Emeralds.—A very fine native emerald has lately been given to this society by the Emperor. Its form is a regular hexagonal prism; it is of a beautiful green colour: one of the planes, which usually terminate the extremities of these prisms, remains in its natural state. The other plane, or base of the prism, is covered by a gangue of micaceous schistus similar, as respects its composition and black colour, to that in which emeralds are found at Herbachthal, near Binsgau in Salzburg; but the crystals of the emeralds have never been found there of such large dimensions as those recently discovered in Siberia, of which the above-mentioned is a specimen. This new vein of emeralds in Siberia is situated eighty-five *versts* to the east of Catherineburg, and was discovered in the following manner. In January last, a peasant of the canton of Belosersk, in looking for stumps of trees to extract resin, found, among the roots of a tree which had been blown down, several fragments of emerald which he sold at Catherineburg. This led to further researches, and a most valuable vein has been discovered.

FOREIGN AND MISCELLANEOUS INTELLIGE

§ I.—*MECHANICAL SCIENCE.*

1. Parabolic Ridges formed on moving Water.

M. Poncelet has made some curious observations on the f the ridges produced when a body is placed in a stream of flowing with uniform velocity. When a point is placed upper surface of water flowing with uniform velocity, a number of ridges appear of a parabolical form. If a stream o spout from an orifice in a vessel, these curves have their ver a line joining the point and the orifice. The summit of t parabola is at the point itself, and is the limit of all the others number of ridges is indefinite; they are placed at distance increase with their distance from the point. These ridges less and less elevated according to their distance from th til they vanish altogether. The ridges are perfectly station invariable in their figure, whilst the motion of the fluid rem same, and they cease to exist the instant the point is remo the vein flow in a trough with vertical sides, the same ph take place as if it were not so confined; and the rid suddenly terminated by the sides without suffering in fl reflexion.

From these phenomena one might at first sight suppose molecules of the current deviate from their natural course a the branches of the curve. This, however, is not the case be proved by throwing fine powder on the liquid vein: the of the powder cross these ridges, and follow the same cou they would take if the ridges did not exist.

In plunging several points into the vein at different dist same system of ridges is obtained for each point, and t cross each other at the points where they meet, without t being in the least altered. When the velocity of the vein twenty-five centimetres (ten inches) per second, the r come imperceptible. They become more and more disti velocity increases. The number of ridges also increase v crease of velocity, the long branches approach more an their common axis. The author remarks, that we hav accurate method of determining the velocity of a curren paring the form of the exterior ridge with those given by with a current whose velocity is known.

If the point be moved in a straight line along the surf water, we have exactly the same parabolic ridges as we s with water flowing with the same velocity as the moving

* Annales de Chimie, xlvi. p. 5.

2. New Theory of Capillary Action.

M. Poisson has published, as a paper, the first part of a work which will shortly appear, and in which he gives his views of capillary attraction. After viewing what had been done before, he arrives at the conclusion that the phenomena of capillarity are due to molecular action, modified not only by the curvature of the surfaces, as Laplace has said, but also by the particular state of the liquids at their extremities due to the deficiency on the exterior of that molecular attraction which exists in the interior *.

3. On the applicable Force exerted by a Horse.

M. D'Aubuisson has examined the useful force of a horse by reference to the effects produced at the Freyberg mines, where the ores are raised by this animal power. The horses belong to the neighbouring countrypeople, and are occupied for eight hours in the day; they are small for draught horses, but in excellent condition.

The power of a horse he distinguishes into useful (or applicable) effect, and dynamic effect: the latter being the total force exerted by the animal, and the former that force minus what is consumed by the resistance and friction of the machine, *vis inertia*, &c. &c. The useful effect is that which it was his object to estimate, and he found it to equal forty kilogrammes raised one metre (or 2.2lbs. raised 39.4 inches) in a second; this being understood of a good ordinary horse working for eight hours, in two portions of four hours each, and in machines of simple construction and properly arranged. From some experiments, &c., of M. Hachette, the dynamic effect would appear to be about sixty kilogrammes raised to the same height in the same time †.

4. Bevan on the relative Hardness of Road Materials.

Mr. Bevan has sent to the Philosophical Magazine a table containing the results of experiments made in 1825, principally upon the hardness of road materials, or their power of resisting the percussion of a given weight of cast-iron falling a few inches upon the several specimens broken to the ordinary size, and resting upon stone or iron. Supposing the weather to have no action, the table would express nearly the relative value of the materials, for the purpose of supporting the wear of a road; and, therefore, those which resist the action of frost and weather, and have the highest numbers, are most valuable.

Mount Sorrel sienite	100
White marble	37. 31
Chert pebble, used much in Middlesex	34. 27. 52. 56. 55. 65

* Annales de Chimie, xlvi. 61. † Annales des Mines, 1830, p. 145.

Quartz pebble in Bedfordshire gravel	7
Ferruginous sandstone of Bedfordshire	20. 4
Hurlock from lower chalk	1
Chalk..	
Granite, Scotch	1
Flint, yellow	33.
Greenstone or basalt, Quittle-Hill near Coventry ...	1
Sandstone, soft	18
Tile fragment	
Gritstone, near Brixworth, Northamptonshire	48.
Limestone, near Bradwall, Bucks................	
Dry clay	
Flint, black	11.
Portland stone, hard	
Quartz, white	
Blue pebble, like Rowley rag	105.
Coarse limestone, near Stilton, Huntingdonshire	
Gritstone, on road near Leeds	100.
Yorkshire paving stone	
Ketton, hard	
Tetternhoe	
Chert? from hills in Devon and Cornwall	
Gray wether, Hertfordshire and Wiltshire	
Grit of upper bed, Collymeston, near Stamford, Lincolnshire	
Second bed do.	
Slate at do.	
Stockton limestone, Warwickshire (lias)	
Newbold, on Avon, do.	
Limestone of Stoke Cruerne, Northamptonshire	

The steady pressure, without percussion, required
piece of the marble weighing half an ounce, was 100 lbs
the grey flint of 1.2 oz. weight, 2000 lbs.; to crush
white quartz pebble of 2 oz. weight, 3400 lbs. *

A specimen of the copper slag, recommended for ro
Fisher of Newgate-street, was sent by Mr. Taylor to
to be tried and compared with the above. Mr. Bev
upon it, that it was the hardest material he had met wi
ber being 234, or above double the highest in the list.
gravity was 4.32. A substance of such hardness, no
decomposition by exposure to weather, and of moder
considered by Mr. Bevan as a most valuable material
great traffic and heavy loads †.

* Vol. ix., p. 164.

† Phil. Mag., N. S.,

5. On the Bur of Perforations.

(*R. W. Fox, Esq.*)

If any slender and sharp-pointed instrument (a common needle for instance) be made to revolve quickly whilst piercing a card, it produces an elevation or bur on each side of it. Hence, may it not be inferred that the same effect, caused by an electrical discharge, is due to the rotation of the electrical current or currents?—for the edges of a hole made by electricity seem to be too regularly and completely elevated on both sides of a card to be reasonably attributed to the simple action of opposite currents not in rotation:—and when several cards are thus perforated together, ought not the outermost cards, on the latter hypothesis, to have the burs on their inner surfaces, and very little, if any, externally?—but it is known that the elevations are generally equal on both sides of the cards: so that the simple mechanical fact above stated appears to strengthen the presumption in favour of the rotatory motion of electrical currents.

6. Condensation of Mercury by Pressure.

It having been deemed very interesting to employ the pressure of the water at great depths for ascertaining the condensation of mercury by pressure, the expedition commanded by Captain Kotzebue was furnished with an elaterometer expressly adapted to this purpose. It consisted of a wide thermometer tube, open at one end, and having attached to it at the other a bulb like that of a thermometer. The tube and bulb were filled with mercury, and a drop of oil poured over it. A scale was attached to the tube, whose divisions showed the thousandth parts of the whole volume of the bulb and tube filled with mercury. When the instrument was made to descend to great depths of the ocean, the greatest condensation which the mercury had undergone during the experiment became visible by the oil adhering to the inside of the tube, even after the mercury had returned to its former state of expansion. At the temperature of 19° C (66°.2 Fahr.), the mercury stood at the zero point of the scale. Let these degrees be $= T$, and those at the depth of the sea $= t$; the expansion, for one degree of the thermometer, of mercury $= m$; of glass $= n$; and the volume of mercury at the temperature $T = V$: then the contraction of the mercury, on account of the temperature, will be $= V, (T-t)(m-n)$, which is to be deducted from the whole condensation observed by the instrument. The following experiment was made in 21° 14′ north latitude, and 196° 1′ west of Greenwich, at the depth of 914.9 toises (5851 English feet). At the greatest depth the thermometer was at 2°.44 C., and the elaterometer marked 3°.1. The condensation was, therefore, 0.0031. Having $T = 19$ and $t = 2.44$, $n = 0.0000274$, $m = 0.000185$, we shall find the contraction of the mercury by

temperature = 0.0026 V, and the compression by the weight water is consequently = 0.0005. Assuming that the conden is proportional to the pressure, we shall find the compression weight of the atmosphere = 0.0000027, which is nearly three as much as 0.000001, the value assigned by Oersted for this pression *.

Note. The great condensation of mercury following fro experiment makes it doubtful, in our opinion, whether some oil may not have insinuated itself between the glass and the m and it would, perhaps, be desirable to supersede the use of by some other contrivance.

7. Instrument for the Condensation of Water by the sure exerted by Water at great depths of the Oce

(*Invented by Professor Parrot of Dorpat.*)

This instrument consists of a hollow glass cylinder terminatin in a hemisphere. The upper end is to be closed by a cove is screwed on. Through this cover a tube, open at the top fixed in so tightly as to allow no air or water to pass. Th when inserted into the glass cylinder, descends nearly to the and, after a bend, reascends again nearly to the top, whe minates in a horizontal tapering piece with a small openin end. The tube is filled with mercury up to the open point, cylinder with water. By a small opening in the cover, again closed when the screw of the cover is quite home, quantity of superfluous water which may be in the cylinde out while the cover is screwing on. It is clear that the pr the water in the ocean is exerted on the water in the cylinde through the medium of the mercury in the tube, and that densation of the water is exactly measured by the quantity cury forced out of the tube through the small opening a The tube is furnished with a scale, on which the proport the volume of the mercury wanting in the tube bears to th of water in the cylinder is read off †.

8. Comparison of the Prussian Weights and Meas the new English Weights and Measures.

(*By Professor Eytelwein.*)

One English inch, at 62° Fahr. = 0.971140 Prussian in Fahr.

An English pound avoirdupois, of 7000 grains, = Prussian loth, 32 of which are a Prussian pound.

An imperial gallon = 253.95383 Prussian cubic inche

* Petersburgh Transactions for 1830.

† 1

‡ Berlin Acad., year 1827, Berlin, 1830.

§ II.—*CHEMICAL SCIENCE.*

1. On the Electro-Magnetic Effects of Metallic Plates having various Positions, Intervals, &c. &c.—(*By M. Bigiou.*)

M. Bigiou has made experiments to determine the relation of electro-magnetic effects, which take place when equal discs of zinc and copper are immersed, under different circumstances, in the same fluid. He obtained the following results:—i. Voltaic electricity is transmitted through a metallic plate, having its surface grooved or roughened with sand-paper, more easily than through a plate of the same metal with a polished surface*. ii. In placing one of the plates inclined to the other, the effect is diminished †. iii. The extent of the surface of copper, as M. Marianini has shown, has a greater influence over the electro-magnetic effect than that of the zinc. The author shows, however, that nothing would be gained in the construction of a voltaic battery, by making the zinc much smaller than the copper, as the greatest effect takes place when they are nearly of the same size. iv. The author remarks that the effect diminishes as the distance between the plates increases ‡. The most valuable part of his paper, is that in which he gives the relative deflecting forces of the same plates with different acid solutions. This was ascertained with the torsion galvanometer.

Water, with	$\frac{1}{80}$ of its bulk of sulph. acid ..	106°	of torsion.
Do.	$\frac{1}{40}$ of muriatic acid	58°	do.
Do.	$\frac{1}{140}$ of nitric acid	106°	do.
Do.	$\frac{1}{80}$ of nitric acid, and $\frac{1}{80}$ muriatic acid,	59°	do.
Do.	$\frac{1}{80}$ of nitric, and $\frac{1}{160}$ sulphuric acid	96°	do.
Do.	$\frac{1}{80}$ of nitric, and $\frac{1}{80}$ sulphuric acid	120°	do.

From this it appears, that equal volumes of nitric and sulphuric acids produce the greatest electro-magnetic effects.

2. Hare's Delicate Galvanometer.

Dr. Hare, of Philadelphia, has constructed a galvanometer, with a riband of tin-foil thirty-four feet long, with a slip of paper intervening, which he says is more sensitive than those made with copper-wire, covered with silk, when the copper-wire was eighty feet long. He finds pure mercury, obtained by precipitating the proto-nitrate by copper, is negative to copper and the other metals, whereas impure mercury is positive, unless the amalgam be formed with the

* According to the experiments of Mr. Leslie, the same thing takes place with heat.

† This is also the case with radiant heat.

‡ He does not seem to be aware of the law, experimentally proved by Mr. Ritchie, (Journal of the Royal Institution, No. I., page 35,) that the effect diminishes inversely as the square root of the distance of the plates.

precious metals. This is a convenient mode of testing the
of mercury. Dr. Hare observed, that when the poles of the
galvanic magnet are brought into contact with the mercury, c
nicating with one pole of the calorimeter, the vertex of the
being in contact with the other pole, a gyratory or whirling
may be observed in the mercury *. The effect is identical w
vortices of Davy, or the rotation of Faraday.

3. POWERFUL ELECTRO-MAGNET.—(*By Professor Henry a* *Ten Eyck.*)

In the last number of this Journal, page 609, we gave an
of a powerful electro-magnet, constructed in America, by P
Henry and Dr. Ten Eyck, which was capable of sustainin
750 lbs. These gentlemen have carried their researc
further, and have actually constructed an electro-magnet
College, which is said to have sustained 2063 lbs., or near
It was constructed on the same principle as the former, but
number of strands of copper wire were employed. 'The m
wound with 26 strands of copper bell-wire, covered wi
thread, 31 feet long; about 18 inches of the ends are left
ing, so that only 28 feet of each actually surround the i
aggregate length of the coils is, therefore, 728 feet. Each
wound on a little less than an inch; in the middle of the
it forms three thicknesses of wire, and on the ends or near
it is wound so as to form six thicknesses.' With a batt
square feet, the magnet suspended 2063 lbs. The effects
battery were not tried. It induced magnetism in a pi
iron, so energetically, as to raise 155 lbs. When two bat
employed so that the poles could be rapidly reversed, a c
was observed. After one of the batteries had been ren
armature, with a weight added, in all 89 lbs., rem
pended, and did not fall when the poles were reversed.
must have been instantaneous, otherwise the weight
fallen, as there was an instant when the magnet could h
power. It was attempted to decompose water by this n
without success †.

4. ON ELECTRICITY INDUCED BY THE RED AND VIOL THE SOLAR SPECTRUM.

Professor Saverio Barlocci, of Rome, states that when t
copper, painted black, and one of them connected with th
of a frog, and the other with the hind feet, are placed o
the red, and the other in the violet ray of the solar spectr

* Silliman's Journal, xx., page 143. † Id. xx., p

brought into contact, that contractions took place in the muscles of the frog *.

5. On the Identity of the Nervous and Electric Fluids.

The following experiment is from an inaugural thesis by Dr. David, of Paris:—'The sciatic nerve of a rabbit was insulated and laid bare, and carefully sponged; a piece of glass was gently introduced between the nerves and the muscles, while the leg of the animal was bent. The sensibility of the nerve was shown by the motions of the animal during the introduction of the needles, the one above the other, but not touching each other. They were placed in communication with the galvanometer: the animal was quite tranquil, and the needle of the instrument at rest. By a sudden movement of the rabbit, the apparatus was deranged, but the needle clearly deviated and moved. The needles were again introduced; some muscular contraction succeeded, again the needle oscillated, but so slightly, as not to convince the assistants. The animal, however, soon made some very vigorous and repeated exertions, and there was no longer any doubt of the fact, for the needle now described an arc of more than two lines. The oscillations ceased with the motions of the animal, and again appeared when it moved. The animal was excited to make contractile efforts, by stimulating the nostrils or irritating the nerve, and the needle immediately oscillated, and the arc it described was great in proportion to the energy of the muscular exertions which were provoked. The phenomena could, in fact, be caused at will. With four needles, double the effect could be produced than when two only were employed. In general, the intensity of the phenomena diminished with the vigour of the animal, and they were not observable after death. When two needles were placed in a nerve, and two in a muscle, the oscillations were barely perceptible; when all four were introduced into a muscle, M. David could obtain no deviation of the galvanometric needle.

Other experiments demonstrated why sometimes the phenomena may not arise when needles were placed in a nerve. The causes of the non-occurrence of the phenomena may be either, i. Insensibility of the nerve, from its being strained or pressed upon in sponging it. ii. Its too great tension over the glass placed beneath. iii. Blood may cover both the nerve and needles. iv. The perfect dryness of the nerve, produced by the sponge. It is then necessary to place the nerve for a moment in contact with the muscles, and its power is restored. It is highly important that the needles and the extremities of the threads of the galvanometer should be perfectly clean.

M. David considers these experiments sufficient, i. To prove that organized beings have a special apparatus, which is destined to

* Journal des Progrès des Sciences et Méd., tom. ii., 1830

furnish an electric current; and ii. To show the c which are required for its production *.

6. Singular Electrical Effect.

'Whilst the workmen were soldering the iron water pi Street,' says the *Winchester Republican*, 'electric shoc duced to such a degree as to cause them to discontinu for the remainder of the day. Several of our citize standing by, got into the ditch and tried the experime effect was the same on all. The pipes are united in t manner:—They are nine feet long, perfect cylinders, w six inches, and a bowl at one end four inches deep; at a funnel pipe, which is inserted into the bowl of th pipe, the spigot end of which is inserted into the bow and so on. When fifty or a hundred pipes are laid manner the process of soldering commences. This is ramming into the joint a few strands of rope yarn, and clay around the joint, leaving an aperture at the top i molten lead is poured; the clay is then taken off, driven home with a blunt chisel and hammer. It w home the lead that the shocks were produced. The s vertical, and the thermometer at 93°, the ditch som and the pipes warm from the action of the sun upon principle is no doubt that of galvanism; but as the posed to be entirely new, the plumber (Mr. Johnson delphia) having never known anything like it during h rience in that city, we should be glad to receive th scientific men upon it. We have since been informe heavy rain on the ensuing day, and the covering of a f pipes some distance above with earth, the phenome occur, nor has it since occurred.'

A correspondent in *Silliman's Journal* considers th doubt thermo-electric, and Professor Silliman agre The voltaic series of iron and lead is supposed to be re by the intense heat of the sun, the black colour of the them to rise to a higher degree than the neighbourin and the pipes being themselves unequally acted upon, a ditch †.

7. Explosion of Phosphorus and Nitric A

Dr. Hare had prepared some very strong nitric acid above one half more of sulphuric acid than the equiv tion), which had a specific gravity above 1.5, and us trate the action upon phosphorus. A tube about sev an inch in diameter, closed at one end, was placed v

* Med. Phys. Journ., 1831, 454. † Silliman's Journ

hollow glass cylinder of about three inches diameter, of which the glass was nearly three-eighths of an inch thick. The whole was situated about four feet in the rear of the table. About five grains of phosphorus, in two or three lumps, was thrown into about as much of the acid as occupied the tube an inch and a half in height. Very soon afterwards, there was a flash, followed by an explosion like that of gunpowder, and the fragments of the glass cylinder, as well as of the tube, were driven in all directions, so as to break many glass articles at the distance of from five to twenty feet, and to wound slightly some of the spectators.

On repeating the experiment on a smaller scale with the same acid, similar effects took place, so that Dr. Hare thinks it necessary to strengthen the ordinary precautions given relative to this action, since it may occasionally rise to such intensity as has just been described.

Professor Silliman adds some statements and remarks, and says, that if the acid be very strong, and especially if warm, the phosphorus burns with a splendid combustion; it is thrown about in jets of fire, and requires great caution. To render it the most beautiful, a tall, narrow, deep vessel should be used, but when the quantity of both substances is considerable, there is sometimes a dangerous explosion. 'This circumstance has happened so often in my own experience with nitric acid distilled from very pure nitre, and *without any water in the receiver*, that I cannot but repeat the caution, that the operator should be much on his guard. With a stick of phosphorus dropped into two or three ounces of strong nitrous acid, I have known an explosion like that of a swivel, and the fragments of glass wounded persons at a distance, although the experiment was performed out of doors, and the spectators, formed into a ring, were none of them nearer than fifty feet, and some who were hit were at double that distance*.

8. On the Phosphorescence of certain Substances.

From the result of numerous experiments made by M. Saladin, he has been induced to conclude that the phosphorescence of many bodies, as dried bones, rotten wood, &c., was caused by the presence of a small proportion of phosphorus evolved by the action of organic matter on the phosphates present, and combining with hydrogen in the nascent state. This action is compared to that upon sulphates in similar circumstances, and has led M. Saladin to several conclusions, and particularly to that which implies that phosphorescence (except in the case of certain insects) diminishes with the proportion of phosphates. Experiments are still wanting to prove this point; but whilst engaged in performing them, M. Saladin is still anxious to secure the credit of priority in taking this view of the effect†.

* Silliman's Journal, xvi. 366. † Journ. de Pharmacie, 1831, 212.

9. On Oxalic Acid.—(*By M. Gay Lussac.*)

Oxalic acid, when heated, is well known to be partly and partly decomposed into carbonic acid and a combu Wishing to know more particularly the nature of these Gay Lussac heated some very pure crystals of the acid i at 209° F. they fused; at 230° water and elastic fluids w gaged, and the latter increased as the water passed 248°—266° the disengagement was very rapid, and until all the acid was decomposed*. The gas was a mixt volumes of carbonic acid and five volumes of carbonic proportion being nearly the same during the whole decon

This easy decomposition was unexpected; for oxalic a sidered as a stable compound among vegetable acids. of sulphuric acid in the ordinary mode of decomposition fore suspected, and it was found, in fact, that when mixe acid, still a temperature of 230°—240° was required. E this important difference, that in the latter case the g consists of equal volumes of carbonic acid and carbonic o

This difference led to the suspicion that some other sub formed during the decomposition. On examination, it that the water which passed over was acid, and that it w the presence of formic acid. At first this acid appear quantity, but it comes over more and more concent towards the end of the operation the product has a very odour and sharp taste. Supposing that the sixth or n lume of carbonic oxide with water forms this acid, then portions of oxalic acid should form one of formic acid, a peared to be the case.

It is evident that the hydrogen has been given to the by the water, and not by the oxalic acid, for then the ca and oxide should have been in equal volumes; besid necessary consequence of the experiments of MM. D Dobereiner. If the decomposition is not carried on too is total, no oxalic acid being volatilized.

These effects render it more imperative, M. Gay Lus that oxalic acid should not be separated from the two pounds of carbon and oxygen, *i. e.* carbonic acid and car It may be ranged amongst those acids into which two of the radical enter, and the name which may be appl *hypocarbonic acid;* but M. Gay Lussac does not press t of this name at present†.

10. Dr. Turner on the Volatility of Oxalic

Dr. Turner having lately examined the volatility of oxalic

* See next page.

† Annales de Chimie, xlvi.

the substance to rise at temperature so low as 212°,* without undergoing any chemical change, except that the common crystals lose two-thirds or two equivalents of their water of crystallization. If ordinary oxalic acid be placed in a water bath, and heated, it effloresces, losing nearly the proportion of water mentioned ; if exposed to the cold air, it recovers the water; but if continued hot, it sublimes, and minute acicular crystals form on the surface. If purified oxalic acid in crystals be exposed to 350° or 400°, in a deep evaporating basin, and, when sublimation begins, the vessel be covered by a layer of smooth filtering paper, a fold of blotting paper, and a larger evaporating basin containing cold water, the oxalic acid condenses in crystals on the filtering paper, or falls on the side of the dish, and after an hour may be removed, and quickly secured in a stoppered bottle. Thus sublimed, the acid is in minute shining acicular crystals, which, on exposure to air, become dull, and regain the two equivalents of water.

At higher temperatures the sublimation proceeds more rapidly. At 300° or 330°, none is decomposed †. At 360° or 400°, the sublimation is very free ; at 414° it fuses, and boils freely; above 330°, decomposition to a greater or smaller extent occurs, and is indicated by the appearance of water. By combining the sublimed acid with bases, &c. &c., its unchanged nature was ascertained.

Dr. Turner found that a saturated solution of oxalic acid at the temperature of 50°, contained 1 part crystallized acid, and 15.5 parts water. At 57°, 9.5 parts of water dissolved 1 part of crystallized acid. At 212°, the quantity of acid dissolved is almost unlimited ; at 220° the crystals fuse in the water of crystallization ‡.

11. Scintillation of Steel. Inflammation of Gunpowder.

You inquire if we have ever tried whether gunpowder will fire in the sparks from our polishing wheels §. We have tried the experiment, and find that when coarse emery is used on the wheels it will be fired at any distance to which the sparks extend ; but when very fine emery is used, a stream of innumerable sparks may be poured upon coarse gunpowder without inflaming it. The same powder, however, on being finely pulverized, will be readily inflamed by the sparks from the fire wheel. In both cases, the sparks are particles of ignited iron, and there can be no difference in the two cases,

* It sublimes at common temperatures. See pp. 73, 74, of the last volume of this Journal.

† See preceding page.

‡ Phil. Mag., N. S. ix. 161.

§ The polishing wheels referred to are of various sizes and kinds, from large grindstones, on which the gun-barrels are ground, to small wheels covered with oiled leather and armed with emery powder. All these wheels are moved with great rapidity by strong water power, and when the steel articles are held upon them, there is a splendid coruscation of innumerable sparks flying off in tangent lines, which follow one another with such rapidity, that the wheel is constantly surrounded with a glory.

except in the magnitude of the particles. It would seem that within certain limits gunpowder will not be inflan ticles of ignited iron, unless they have at least a certain in relation to the magnitude of the grains of the pow question was probably suggested by the fact, well kno that on putting the hand into the stream of sparks, th experienced is rather that of *cold* than of *heat*. This is a not a little surprizes those of our numerous visitors wl courage to present their hands to a stream of fire so have the appearance of one continued flame. The parad hend, may be explained in the following manner.

The particles which make up the stream are much small sions and fewer in number than they appear to be, ea from the extreme rapidity of its motion, appearing to ext inches, when, in fact, it is little more than a mere po particles, being thus minute, do not impart a sufficient heat to penetrate through the insensible external memb skin, called the cuticle or epidermis, so as to reach the adj brane which alone is the organ of sensation, before withdrawn by the increase of evaporation produced by of air which the wheel puts in motion. If the hand is h in the stream until the evaporation is diminished by t desiccation of the skin, we shall perceive a mild sensat These sensations, first of cold only, and afterwards of take place only when we present to the stream the *inside* or fingers, where the cuticle is thick. If the *back* of t presented, a very pungent and pricking sensation of h duced at every point where a particle impinges, highly at the same time, with a general sensation of cold, prod increased evaporation. In the first case, the heat is pass the thick cuticle of the inside of the hand, extends l loses its intensity before it reaches the sensible membra cuticle on the back of the hand being extremely thin is penetrated *.

12. New Pyrophorus.

Dr. Hare recommends as a new pyrophorus, Prussian to redness, for about a minute, in a glass tube, and th hermetically. As soon as the tube is fractured, and t thrown out upon a table, they take fire †.

13. Crystallization of Oxides of Iron and Z

(*By M. Haldat.*)

M. Haldat is in the habit of using a bundle of soft iron demonstration of the action of steam at high temperatu

* Silliman's Journ., xvii. p. 114.

† Ibid., xix

and by management has obtained larger crystals of oxide of iron than are usually procured. These crystals are finer the longer the action of the iron and steam has been continued; they have occasionally been obtained one-tenth of an inch in size upon large wire or plates of iron. The crystals are very brilliant, and, under the microscope, perfectly resemble those from Elba or Framont. They are usually rhomboids, covering each other as in the iron ores from those countries, have the same brilliancy and colours, and every other character except size.

M. Haldat then endeavoured to obtain crystallized oxide of zinc by similar means; and by using precautions relative to the application of heat, rendered necessary by the greater fusibility of zinc, succeeded in his object. The oxide had two forms, being sometimes in amorphous globules, and at others in plates covered with rhomboidal transparent crystals, having the colour of honey.

As in volcanoes, most of the circumstances meet necessarily in these experiments to produce crystals, it is probable that all the varieties of crystallized oxide of iron there found, result from a process analogous to the present *.

Mr. Daniell obtained very fine octohedral crystals of oxide of iron upon the bars of iron, used in his pyrometrical experiments, and which had been heated with very imperfect access of air †.

14. Discovery of Vanadium in Scotland.

Mr. James F. W. Johnston has discovered Vanadium in Scotland, in a mineral from Wanlockhead, resembling in appearance an arseniate of lead; and, it is a remarkable circumstance, that this new substance has been discovered by three different persons—Professor Del Rio, Professor Sefstrom, and Mr. Johnston—in three different countries, Mexico, Sweden, and Scotland, nearly at the same time, and without any knowledge, on the part of one, of what the others had done.

Mr. Johnston discovered it during the last winter in two minerals, very different in character, but both compounds of lead, probably Vanadiates. The first resembles an arseniate, occurs in small mamillæ upon the surface of calamine, sometimes passing into a crystalline form. The second mineral can hardly be distinguished from earthy porous peroxide of manganese. It occurs amorphous and in small rounded forms, often powdering the calamine with a thin black coating, and at times scattered in cavities ‡.

15. Yellow Dye from Sulphuret of Cadmium.

M. Lassaigne proposes to use this substance as a yellow dye on silk. If the silk be immersed in a solution of chloride of cadmium for fif-

* Annales de Chimie, xlv. 70. † Phil. Transactions.
‡ Brewster's Journal, v. 167.

teen or twenty minutes, at temperatures between 122° and then wrung out and immersed at common temperatures, i solution of hydrosulphuret of potassa, it becomes of a yellow colour, or of lighter or deeper tints, according to the of the cadmium solution. This dye is unaltered by sun weak acid, or alkaline solutions. Wool could not be dyed of this substance with the same facility as silk*.

16. Separation of Antimony and Tin.

The separation of these metals is very difficult, because of larity existing between them. M. Gay Lussac has for thi long successfully used tin as a precipitant of the antimo two metals, their weight being known, are supposed to b tion in muriatic acid. If alloyed, they would be dissolve atic acid to which small quantities of nitric acid had bee sively added. A plate of tin is to be immersed; and th muriatic acid in excess, the antimony will soon deposit powder. The effect will not be perfect at common tem but by applying the heat of a vapour-bath, it will quicl cluded, provided that excess of acid is continued in the liq antimony is then to be well washed and dried on the If the two metals are in solution, their weight not bei one portion should be precipitated by zinc, to give th both metals, and another portion by tin, to give the pr antimony†.

17. Compound of Bi-cyanide of Mercury and Iodide sium. (*By Dr. Apjohn.*)

When solutions of iodide of potassium and bi-cyanide are mingled and left, a beautiful pearly substance, in ve sided prisms, is formed, very soluble in hot water, affected by water at 60°. These undergo no change b potash, or the carbonated alkalies in solution: muriatic them bright scarlet, and evolves the odour of prussic a ignited in a crucible, the residue gave an abundant pre tartaric acid. These and other experiments proved t pound contained both the salts added in solution, and u they were found in the following proportions:—

Bi-cyanide of mercury . . .	24.
Iodide of potassium	15.

And as these numbers are very nearly in the ratio of t numbers, it appears that a proportional of each proxi is present. That the substances are not present, as and hydriodate, is shown by the correspondence of weights with the estimate above given, and by the fa

* Annales de Chimie, xlv., 433. † Ibid., xlvi.,

heated in a tube, no water is evolved. Dr. Apjohn proposes to call the substance iodo-bicyanide of potassium and mercury*.

18. Composition of Tartaric Acid.

By differences between his own estimate and that of Prout, Berzelius has been induced to re-examine the composition and number of tartaric acid. He finds, from many experiments, that the tartrate of lead is composed of

Tartaric acid	37.2569
Oxide of lead	62.7431

Its atomic weight, therefore, is 828.05; hydrogen being 12.5, and oxide of lead 1394.5.

Tartaric acid is composed of

Hydrogen	3.0045
Carbon	36.8060
Oxygen	60.1895
	100

which agrees with 5 atoms oxygen, 4 carbon, and 4 hydrogen (on the assumption that water contains 2 atoms of hydrogen). Calculated in this way, the atomic weight should be 830.709, instead of 828.05, as above. Prout made it 830.709 by his analysis.

Berzelius then examined the equivalent number of lead and its oxide very rigorously; and for this purpose prepared an extremely pure oxide of lead, and reduced it by hydrogen at a sufficiently high temperature. From the mean of six experiments, the results were, per cent.—

Lead	92.8277
Oxygen	7.1723

And the atomic weight of lead 1294.29, instead of 1394.5, as above; hydrogen being 12.5.

All the six results were between 1293 and 1296. 'But if the atom of hydrogen weighs 12.5, the atomic weight of lead, to be a multiple of that number, should be either 1287.5 or 1300; and if one of these numbers is the true one, my results ought to oscillate about it, instead of oscillating, as actually happened, between 1293 and 1296.' Other considerations, drawn from the analysis of the tartrate of lead, are by M. Berzelius considered as opposed to the theory of multiple numbers of hydrogen †.

19. Racemic Acid or Para-tartaric Acid.

The following account of this acid is abstracted from Berzelius. A manufacturer of tartaric acid at Thann, on the Upper Rhine, first observed that with the ordinary acid appeared crystals of another

* Phil. Mag., N.S., ix., 401. † Annales de Chimie, xlvi.

and less soluble acid. He mistook it for, and sold it as, o
In 1819, Jahn described it in his Dictionary of Chem
distinguished it from both tartaric and oxalic acid. In
Lussac showed that it was not tartaric acid, though its
number was almost the same. Some time after, Walchn
perimented on this acid.

The acid is supposed to be peculiar to the grape of
Rhine; but is probably present in all grapes. It may b
by exactly saturating tartar containing racemic acid, with
of soda, and crystallizing most of the double salt forr
tartrate separates first, the far more soluble racemate
dissolved; when it crystallizes, its forms are different
the tartrate. The mother liquor is to be evaporated, p
by a salt of lead or lime; and the separated precipitate d
by sulphuric acid. Racemic acid first precipitates in cr
the acid solution, and then tartaric acid. Racemic acid
parts of water for its solution; tartaric acid only 2 parts.

The equivalent number was deduced from the racemat
This compound is much more soluble in excess of acid th
of lead, and usually covers the sides of the glass with a tl
line crust. The precipitated salt does not contain wate
crystallized salt does. The results of analysis were
same, both for the atomic proportion and the ultimate co
as for tartaric acid, (for which see the preceding page;)
substance furnishes a new example of the extraordina
bodies composed of the same number of simple atoms
theless, possess different properties.

Racemic acid has a different crystalline form from tarta
also effloresces by heat, which tartaric acid does not.
racemic acid, exposed to dry air at 64° F., for twenty-
was then dried in a current of air at 212° F., and los
cent. of water. This loss did not increase, and it was f
sary to use oxide of lead as a base, and apply heat
before all the water was expelled. The loss ultimatel
to 21.35 per cent., or two proportions of water. Of
may be disengaged by heat alone, and the other by heat
As tartaric acid also contains two proportions of water
ence in the characters of the crystals is not due to that s

With potash, an acid racemate may be formed equa
with cream of tartar, and containing, like it, an atom
crystallization. The crystals appeared to differ in form
of cream of tartar. The racemate of potash and soda,
all, does not resemble Rochelle salts: a confused mass
and it is uncertain whether it is a double salt or a mix
A racemate of potash and antimony may be formed a
tartar emetic, but its crystals differ, being sometimes
sometimes rhomboidal prisms. As two tartrates of
antimony can be formed, one crystallizable, the other

two similar racemates exist, but both are crystalline, the second occurring in acicular crystals, which in sunshine become as white as milk. The same change happens to the corresponding gummy tartrate, in which also minute crystals may occasionally be seen.

The racemate of lime is much less soluble than the tartrate; both have the same composition, and both contain 4 atoms of water in combination. The tartrate contains 21.765 per cent. of lime, and the dried racemate gave 21.775 per cent. of lime. A solution of sulphate of lime in water is decomposed by the addition of a little racemic acid; and after twenty-four hours, most of the lime is found precipitated as racemate of lime. Tartaric acid does not do this. If two solutions be prepared in muriatic acid, a little diluted,—one of racemate of lime, and the other of tartrate—and then each be saturated with ammonia, the racemate quickly falls as a semi-crystalline, white, opaque powder; but the tartrate does not, unless the liquid be much concentrated. After some time, octohedral crystals form upon the glass. This is a good method of distinguishing the acid, when one of them is in solution. A solution of racemate of lime, evaporated spontaneously, yields crystallized racemic acid; but evaporated by heat, the muriatic acid is volatilized, and the addition of water dissolves no racemate*.

20. On Gallic Acid.—(*By M. Braconnot.*)

M. Braconnot had recommended the preparation of gallic acid by a process in which the tannin present in the infusion of galls was removed by gelatine; but M. Berzelius thought that such gallic acid was chemically combined with tannin, and that pure gallic acid could be obtained only by sublimation. M. Braconnot has therefore made further experiments, and finds that the two substances differ, and that, not from the presence of tannin in the unsublimed acid. The latter he calls pure gallic acid, and the other pyrogallic acid.

When very white gallic acid, giving no indication of tannin to gelatine, was moderately heated, it became a brown liquid, which crystallized on cooling, and which, dissolved in water, contained still gallic acid and a brown substance which precipitated gelatine abundantly. Thirty parts of dry white gallic acid, being subjected to a higher heat, gave only 3½ parts of sublimed gallic acid: though very white, its solution precipitated gelatine. The residue, when dissolved, gave a brown liquor, which became much deeper with persulphate of iron, and blue-black with protosulphate (these being characters of pyrogallic acid, and not of gallic acid); and it also abundantly precipitated gelatine. Hence, heat appears to rearrange the elements of gallic acid, so as to produce a peculiar variety of tannin and pyro-gallic acid.

Pyrogallic acid reddens litmus paper, though less than gallic acid;

* Annales de Chimie, xlvi., 128.

it has a cool, bitter taste. It dissolves in $2\frac{1}{2}$ parts of wa F., whilst gallic acid requires 100 parts at the same te When re-sublimed, pyrogallic acid is decomposed almo producing tannin and charcoal. It dissolves in ether. I solution is colourless, but by exposure to air becomes col deposits ulmin, being entirely decomposed in a few days added as evaporation proceeds. Persulphate of iron ad immediately reduced to protosulphate, and a tanning formed. Protosulphate of iron produces a blue-black liqu actions are very different from those of gallic acid, which salts of iron produce no change, and with persalts pro blue colour.

Pyrogallic acid slightly heated with strong sulphuric not become coloured, and is not sensibly decomposed. acid treated in a similar way became coloured, but on ad was found not much altered; no tannin was produced. phuric acid and a higher temperature converted the gall ulmin; no tannin was formed.

Pyrogallate of alumina forms a bitter solution, becor by heat, and transparent when cold; it powerfully coag tine; crystallizes; and reddens litmus paper more pow the acid alone; as if alumine itself acted as an acid.

Every endeavour to form gallic acid (upon Berzelius' combining pyrogallic acid and tannin, failed. From all M. Braconnot concludes, i. That gallic acid procured in way, and cleansed by animal charcoal, is pure; ii. Tha verts it into tannin and pyrogallic acid; iii. That gallic be produced from tannin and pyrogallic acid *.

21. Sulpho-Sinapisine, or Sulpho-Sinapic A

MM. Henry and Garot described some years since a cur derived from white mustard-seed (*sinapis alba*), which sulpho-sinapic acid, to point out at once its source, t of sulphur in it, and its acid nature. Some discussion relative to the subject, M. Pelouze having denied the this acid, and referred the properties observed to t of sulpho-cyanic acid or its compounds. This has a memoir from the discoverers and from others, in curious points relative to this body are established. W tend to enter into the controversy, but wish to give an ac points really established, or, at least, most recently asce following account is from MM. Henry and Garot's secon

Sulpho-sinapisine is prepared by boiling the coars white mustard-seed, or of the *turrites glabra*, for a five or six times its weight of distilled water, in a coppe liquid passed through a cloth, and the solid portion pr magma, when exposed to the air, evolves a sulphurous

* Annales de Chimie, xlvi., 206.

yellow tinted liquid is slightly acid; it is to be quickly evaporated in a tin vessel by a water-bath, until like honey, when it will appear as a yellow, bitterish mass, having an odour like that of osmazome. Six or eight times its weight of strong alcohol is immediately to be added; and a tincture will be obtained yellow, slightly acid, and producing a strong red colour with persalts of iron. This tincture being distilled, the product has no power of affecting persalts of iron. The red-brown, syrupy, transparent matter left in the retort, crystallizes by slow cooling, or becomes a granulated mass if agitated: being allowed to drain on a cloth, then pressed, then purified two or three times in very strong alcohol, its bulk increases much, and it ultimately appears as a white crystalline substance, and is *sulpho-sinapisine*. Further portions may be obtained by evaporating the mother liquor: the crystalline masses obtained require to be digested in ether to separate a volatile red body soluble in that fluid.

Sulpho-sinapisine is white, inodorous, bitter, very light, soluble in water and alcohol, and more so when hot than when cold. When hot saturated solutions are cooled, acicular crystals in rounded groupes are produced; they are most distinctly formed in acidulated water. When heated, it fuses, and is decomposed, yielding fetid products, amongst which are carbonate and hydro-sulphuret of ammonia. The substance is not naturally acid, but yields an acid by various modes of treatment.

Being analysed, its ultimate composition appeared to be per cent.

Carbon	50.504	or by theory	50.504
Hydrogen ..	7.795	......	7.795
Nitrogen ...	4.940	......	5.020
Sulphur....	9.657	......	9.657
Oxygen	27.104	......	27.024

As the nitrogen and sulphur are nearly in the proportions belonging to sulpho-cyanogen, we may consider sulpho-sinapisine as represented by sulpho-cyanogen, and an organic matter not azoted, which may perhaps be competent to form the volatile oil of mustard.

Sulpho-Sinapisine, with nitric acid, is instantly altered; a bright red colour is produced, much orange-coloured gas formed, and sulphuric acid appears. With muriatic acid it is dissolved, and becomes green; heat then produces a strong odour of prussic acid. Distilled with sulphuric or phosphoric acid, sulpho-cyanic acid is obtained, and, with the first, sulphuretted hydrogen also. With chlorine, prussic acid and sulphuric acid were formed.

When treated with alkalies the following were the principal results. Ammonia dissolved it, but effected little change. Potassa and soda, with a little water, changed the colour to red and green; being evaporated and dried, much volatile oil of mustard rose. Then the substance fused, and was found to be sulpho-cyanide of potassium; further heat destroyed this state of things, leaving

charcoal and sulphates. Lime and baryta produced similar heat enabled the mixture to produce volatile oil of mus sulpho-cyanogen.

As to the action of salts, persalts of iron are strongly rec *sulpho-sinapisine.* The persulphate of copper is precipitat the proto-nitrate of mercury is precipitated white; the acid silver is precipitated as a dense white substance, the weight as sulpho-cyanide, is equivalent nearly to the sulpho-cyanog the substance could form. From all these circumstances that sulpho-sinapisine is a curious body, not acid, conta phur, and containing no sulpho-cyanogen, but competent by various modes of treatment *.

In an after Number † is a highly interesting memoir on subject by MM. Boutron and Robiquet. Suspecting th sequence of the presence of water, certain substances we during the process of extraction, which did not pre-exist in tard-seed, they devised various processes dependent upon of ether and strong alcohol, (and which involved muc to obtain the substances in the seed in a state as near th condition as possible, gathering up as they proceeded nu servations on the changes, &c. &c. of the substances duri cessive actions. By the action of ether, in the first plac covered an acrid fixed principle, which had not before bee On carrying on the operations they obtained the sub responding to the sulpho-sinapisine already described; b very different composition to that given. In the following c second gives the proportions of the new sulpho-sinapisin that of the substance before described, the numbers bein by M. Pelouze.

Carbon........	54.0000	...	57.920
Hydrogen......	10.6512	...	7.795
Nitrogen	2.8392	...	4.940
Sulphur	9.3670	...	9.657
Oxygen	23.1426	...	19.688

From these and other anomalies they conclude that i sible to state any thing correctly respecting the tru organic substances, and also that we cannot be certain existence, until they have been obtained identical in th several different processes.

On mixing black mustard powder with its weight of or with water containing a little acetic or sulphuric acid bonate of potassa, the two first mixtures evolved thei powerful odour, but the two latter were inodorous. latter cases the effect is due probably to some kind of c

The conclusions at which these authors arrive are chemical composition of the seeds of white and blacl essentially different. ii. That the active principle of w

* Journal de Pharmacie, 1831, 1. † Ibid. p

is a substance not volatile, not pre-existing in the seed, and which may be due to sinapisine, combined with some other product; for when once the latter is separated, the former is not produced. Both contain sulphur. iii. The active principle of black pepper is a volatile oil, having no pre-existence, and not capable of development without the contact of water. iv. There is probably a principle in this seed from whence the sulphur found in the volatile oil is derived. v. *Sinapisine*, extracted by alcohol, without the intervening action of water, has not the property of reddening the persalts of iron, nor of evolving odour by the action of caustic alkalies. It is less soluble in alcohol, and contains less nitrogen than the sulpho-sinapisine of M. Henry and Garot, but it contains sulphur essentially.

In the same Number is a memoir by M. Fauré on the same subject, in which he also shows the important part performed by water, in contributing to the formation of those substances which characterise black pepper, and make it valuable in medicine. In every pharmaceutial preparation into which mustard powder enters, he considers it essential that as much taste and odour should be developed as possible, and that the powder should first be mingled with water, and then the other substances added. Acids and alkalies do not add to its irritating effects, except perhaps by those which they themselves possess. He arrives at the following chemical conclusions:—i. The volatile oil of mustard does not pre-exist in the seeds or powder; water is indispensable to its formation. ii. Besides the well known principles in black mustard, it appears to contain a particular green substance, which appears to aid in forming the volatile oil. iii. Sulpho-sinapisine is one of the principles of black mustard, and accompanies the green matter in almost every operation to which the seed is subject. iv. Ether has no marked action on the constituent elements of the volatile oil. v. Rectified alcohol, and weak acids and alkalies, when added to mustard powder, oppose the formation of the volatile oil.

22. Salicine.

MM. Herberger and Buchner have found salicine to be a sub-salt, containing a true vegeto-alkali. The *salicia*, or base, has all the properties of those bodies, except that it dissolves with facility in water. When burnt it left no residue. It differs from salicine by its alkaline action on litmus paper; by its crystals, which are nevertheless prismatic; by its solubility in water, and by its action on concentrated and dilute acid. It is not so soluble in alcohol as salicine. The sulphate, nitrate, phosphate, acetate, tartrate, oxalate, and muriate of salicia, have been made; ether does not dissolve them, alcohol does, and upon evaporation leaves them (if there be not enough water of crystallization) in a pulverulent or flocculent form. When heated the salts fuse; then become dry; and when more highly heated, fuse and are decomposed with the odour of quinia in

* Journal de Pharmacie, 1831, p. 299.

combustion, leaving a bulky charcoal, which ultimate away.

The saturating power of salicia is very small.

All the salts, except the acetate, crystallize in needles, f representing vegetation. The acetate assumes the granu Many of them effloresce; their taste is generally bitter.

The salicia was obtained by dissolving salicine in a so oxalic acid; this acid was then separated by lime, &c. &c.

The acid part of ordinary salicine was then separated b it with phosphoric acid at a moderate temperature. This is volatile, and may be obtained by distillation. It has al perties of a sub-acid, and is the cause of the aroma of salicin salicia has no odour of the kind. Pure salicia has no ac over ordinary salicine *.

23. On Draconine.

M. Melandri announced the existence of a vegeto-alkali in blood, which he called draconia. M. Herberger has obt substance in a state of purity, and states that it has no c thus ranked, but that it possesses the properties of a sub- should be classed with tannin, &c. &c.

Dragon's blood consists of	fatty matter......	2.(
	oxalate of lime...	1.(
	phosphate of lime.	3.'
	benzoic acid.....	3.(
	draconine.......	90.'

24. Precipitation of Morphia from Laudanum. I

'I believe it is not generally known that the addition of a alcohol to common laudanum will cause a crystalline pr morphia in the course of a few hours. If the precipitat tained be dissolved in acetic acid, again precipitated by and afterwards collected and dried upon a filter, the morp obtained nearly white, and may be rendered perfectly so ing the solution by acetic acid and precipitation by ammon by these means obtained thirty grains of morphia from a opium.

'Instead of alcohol impregnated with ammonical gas, a equal parts of strong aqua ammoniæ and common a answer.

'Narcotin is, I find, sometimes spontaneously precip crystalline form, from a solution of opium in proof spirit. T stances under which I procured it are nearly these:—a pound of opium was boiled in a quart of proof spirit, an while warm, through a coarse cotton cloth; the sol obtained being allowed to stand for about twenty-four hou were observed to be spontaneously deposited on the sides taining glass jar. These being dissolved in acetic acid,

* Journ. de Pharmacie, 1831, 225.

† I

dition of ammonia, a precipitate took place, which was collected by a filter and dried. Narcotin was thus obtained in the form of white and beautiful silky crystals, which were readily soluble in sulphuric ether.

'When we consider how often opium has been dissolved in proof spirits by chemists and pharmacopists, it is surprising that crystalline principles, so easily evolved, as are morphia and narcotin, by the process above described, should have escaped observation until lately, when Sertuerner, by a much less obvious route, had the honour of discovering them*.'

25. Colouration of Azoted Bodies by Nitrates of Mercury.

M. Lebaillif remarked that when certain vegetable and animal substances were moistened with a solution of mercury in nitric acid, they became of a red or amethystine colour, but the effect did not take place with the protonitrate or pernitrate of mercury separately. He and M. Lassaigne investigated this action, and found that a solution containing both prot and per nitrate always produced the effect. Such a solution is always produced when mercury is dissolved by moderate heat in nitric acid; the production of colour is so ready, that if a piece of animal matter, as dried white of egg, caseum, horn, &c., be moistened with the mercurial solution, it will be reddened in eight or ten seconds, and if warmed (as upon platina foil, six or eight inches above a candle), will take a rich purple red colour. The effect may be produced even with milk, mucus or dissolved gelatine.

Organic substances containing azote were such as, in this way, became coloured. Thus starch was not coloured. Gluten was readily coloured, and it was found easy to find out very small quantities of gluten in starch by the rose tint then acquired: but still all organic azoted substances were not coloured. Fibrin and the varieties of albumen, including caseum, horn, wool, milk, membranes, &c., gelatine, silk, vegetable albumen, wheat flour, sweet almond, and some other substances, became coloured. Urea, uric acid, osmazome, picromel, sugar of milk, sugar, starch, lignine quinia, morphia, and the vegetable acids, were either not coloured at all, or only pale yellow.

A solution made of one part of mercury in two parts of nitric acid was boiled for four or five minutes, to convert part into pernitrate, then diluted with its bulk of water, and silk or wool immersed for ten or fifteen minutes, at temperatures from 113° to 122° Fah.: it was thus dyed of an amaranth colour, more or less deep, and which, upon the silk, withstood the action of light, and of weak sulphuric acid or alkaline solution. The colour appears due to a combination of the mercurial salt, for the silk becomes brown by a solution of hydrosulphuret; and 100 parts of white silk increased by 17 or 18½ parts during dyeing†.

* Silliman's Journal, xvi. 365. † Annales de Chimie, xlv. 435.

26. Tanning of Leather by Grape Marc.

A medical man of the neighbourhood of Narbonne has a that the marc of grapes, after being distilled for the p separating the alcohol, is an important assistant to oak ba tanning process. After preparing skins in the usual n placed them in the pits with the marc, in the place of thirty-five or forty days the tanning was finished. The advantages are, i. shorter time; ii. reduction of the pri bark: iii. a more agreeable odour of the leather than tha oak bark; iv. greater strength in the leather *.

27. Analysis of a Salivary Concretion, by Professor of Dorpat.

The calculus in question was one inch and a half in l twenty-eight grains in weight, and consisted of a great concentric layers; it was examined in the following man digestion of fifteen grains of the powdered substance witl ether yielded a colourless extract, by the evaporation of eighth of a grain of yellowish white fatty matter was obtai had no taste or smell, was insoluble in water and alcoh being incinerated exhibited a slight trace of iron. The r the powder from the digestion with ether was boiled wi the liquid evaporated, and half a grain of a yellowish obtained, half of which was soluble in water; the solut a weak saline taste, and had the odour of osmazome; i cipitated by the oxalate of ammonia and nitrate of s other half, which was not dissolved by the water, was ether, and exhibited all the properties of the ethereal ex remainder of the powder being boiled with water, a colo tion was obtained, which, during evaporation, becam colour, and yielded one grain and a half of a dry brownis which was perfectly soluble in water, but not soluble i coloured by, ether or alcohol. The red colour of the sol very much resembled that of the sulpho-cyanate of iron, destroyed by adding a few drops of liquid ammonia or m and as the incinerated mass of the brown residuum, been heated with a few drops of nitro-muriatic acid, an in water, was precipitated by the tincture of galls, of a b by the ferro-cyanate of potash, of a blue, and by the sul of potash, of the original red colour, there can scarcely b that it proceeded from the presence of sulpho-cyanate order to ascertain this still more clearly, nine grains stance were boiled with distilled water, and on adding of the solution of chloride of iron, the liquid was found to become of an intense red colour. No trace of pota

* Recueil Industrielle, xvi. 85.

found, but there was a considerable portion of soda, and it is accordingly reasonable to suppose that the sulpho-cyanogen had originally been combined with sodium, and only during the evaporation become united to the iron. The residuum from the powder which had been successively submitted to the action of ether, alcohol, and water, was now heated with muriatic acid, which formed a transparent solution, and an insoluble gelatinous substance of one-sixth of a grain in weight, after having been dried. The solution was saturated with ammonia, and yielded twelve grains and a quarter of phosphate of lime, with a small quantity of animal substance, as appeared from the smell during combustion. The remainder of the solution being mixed with oxalate of ammonia, formed a precipitate of oxalate of lime, which, on being ignited, was found to be one-eighth of a grain; besides there were some traces of carbonate of magnesia. Fifteen grains of the salivary concretion would accordingly consist of—

0.375	of fatty substance,
0.250	„ osmazome,
1.500	„ Sulphates, iron, chloride of calcium, sulpho-cyanide of sodium,
0.166	„ Animal substance (mucus?),
12.250	„ Phosphate of lime,
0.212	„ Carbonate of lime,
0.247	„ Carbonate of magnesia, water, and loss.
15.000*	

§ III. *NATURAL HISTORY.*

1. Power of Carbonic Acid on the Lungs.

When M. D'Arcet went to visit the very abundant and curious source of carbonic acid, existing at Montpensier, in the department of Puy de Dome, he endeavoured to ascertain personally the effect of the gas when respired. He kneeled down, therefore, near the larger source, supporting himself on his hands, and advanced his head slowly downward, intending to raise himself the moment he felt any indication of risk: but on commencing the respiration of the gas, the effect of feebleness and extinction of power was so sudden, that he fell down flat, with the face entirely immersed in the current of carbonic acid, and would have lost his life, but that the guide whom he had forewarned, raised and carried him away to the fresh air.

M. D'Arcet proposes two curious uses of the place. The nature

* Schweigger-Seidel's Jahrb. 1830, iv. 403.

of the ground, assisted by certain protecting hedges, will en carbonic acid to collect in large quantities. A cistern formed at the lowest level, and then when animals come to water, or are tempted by the green shade, they will be ki thus much game is calculated upon for the advantage of th Then a house is to be built with an inclined floor, a pully, rope, &c., so that a dog may be tied to the rope, led into bonic acid atmosphere in the house, rendered insensible, h again and revived by the fresh air; and thus by making the c experiment of the Grotto del Cane in a scientific way, m pany, it is expected, will be drawn to the place*

2. On the Phenomenon of Blushing.

M. E. A. Lauth observes, that he is not aware that any p formation has been afforded as to the kind of vessels whicl the colour of the face. Most physiologists merely say that upon the capillaries. M. Lauth states, that if the arteries cessfully injected, the whole of the face becomes of an ur tint. It cannot, therefore, be these vessels which produce nomenon of blushing. He has derived the following resu perfect injection of the facial veins: the cheeks we coloured, the chin, the tip of the nose and the forehea a slighter tint, and the other parts of the face were still les This kind of colouration resembles that which is produce tal emotions during life, and we may therefore conclude ing depends in part upon venous congestion †.

3. General Emphysema formed by a combustibl

This singular case was described by M. Bally, at the Royale de Médecine. A man twenty-five years of age, wl ill fifteen days, was admitted into L'Hôpital Cochin, v toms of typhus fever. He also complained of severe left thigh, and whilst he was in a state of delirium, he s been bitten on the knee by a dog. The limb was most examined, but not the slightest trace of such an accide discovered. The thigh and scrotum were much swoller the following day. *Dissection eight hours after death.* of the body was soiled by blood, which had also transu the integuments of the thighs: some blood had also charged from the nose. The whole body was emphyse the left inferior extremity was in this state to a very h it was double its natural size, of a brown colour, and numerous phlyctenæ, some black, of great extent and clusters, from which escaped a reddish serous fluid, mi quantity of gas; others were white, and from these not escaped.

* Recueil Industriel, xv. 220. † Mem. de la Société, &c.

The limb sounded upon percussion, and when it was pressed with the hand crepitation was distinctly heard; the abdomen was much distended with gas: the face and temples were deeply injected with blood, and were of a violet colour. Upon dividing the scalp a large quantity of dark red blood escaped. The nerves and lungs presented no remarkable appearances, heart pale, and void of blood. In the intestines were observed those alterations which are so commonly detected in cases of typhus fever. Bubbles of air filled the vessels of the pia mater and the left vena saphena. The lymphatic ganglions of the mesentery were enlarged, and contained gas, which took fire from the flame of a taper, and produced an explosion. The same phenomena also followed the exit of air which was contained in the legs, thighs, and scrotum, where incisions had been made into these parts. A puncture was made in the abdomen, and the gas which escaped also took fire and burned for some time; the flame was blue at its base and white at its summit. The combustion extended to the puncture which had been made with a trocar. The edges of this aperture became black, and were consumed, and the aperture itself was enlarged to double the size it had originally been made. The gas which was contained in the subcutaneous cellular tissue of the thorax was equally inflammable *.

4. Poisoning by mouldy Bread.

Dr. Westerhoff attended, in 1826, upon two children of a labourer, who had been simultaneously attacked with the following symptoms. The eldest, ten years of age, had his face red and swollen, his countenance was animated and bewildered, tongue dry, pulse feeble and quickened, head-ache, giddiness, unextinguishable thirst, violent cholic, desire to sleep, and alternate unsuccessful attempts to vomit; subsequently sudden vomiting and very abundant alvine evacuations, after which very great faintness, indifference to everything, and sleep only for a few minutes at a time. The younger, eight years of age, was even more violently attacked. Having understood that they had eaten the preceding day only a piece of old mouldy rye bread, Dr. Westerhoff prescribed a demulcent treatment, and they soon recovered.

Some time afterward, several boatmen having eaten some mouldy rye bread were attacked with similar symptoms, but they were quickly relieved by vomiting, which came on spontaneously. The question suggested by these cases is, whether this kind of poisoning arises from an alteration in the quality of the bread, or from the vegetation which constitutes mouldiness (mucor mucedo) †.

5. Cure of Scrofula by Iodine.

A report, expressing the utmost approbation, has been made by MM. Dumeril and Majendie to the Académie des Sciences at Paris, upon M. Lugol's application of iodine, in scrofulous cases, at the

* Med. and Phys. Jour., 1831, p. 514.

† Archives Générales.

hospital of St. Louis. They state that that very comm dreadful disease, which was formerly considered as incurab by a rigorous rule, subjected its sufferers to exclusion from t pitals, may now be cured; that it is not merely cases in th stage, but those that were exceedingly advanced (scrofulo sumptions), that were cured. Where the glands, organs, tions, and bones had suffered greatly, still a month sufficed the patients. M. Lugol has operated only in the worst cas as, having no hope, came to the establishments to die. 'M does not claim the discovery of the utility of iodine in scrof by the great number of cures which he has effected by his perseverance, and by the light he has thrown upon the inte external application of iodine, in various states of prepar Lugol has advanced medical science an important step.' ' demy approved the report *.

In 17 months, 109 scrofulous patients had been tre iodine only. At the end of 1828, 39 were still under the pl hands, 30 had left the hospital much improved: with 4 t cation had been useless; 36 were perfectly cured †.

6. On the Use of the Secale Cornutum.

The following general results, obtained by Dr. Villeneu cases, are quoted by Dr. Armour in a paper upon this i. In 600 the success was complete in cases of labour, p called, *i. e.* for the expulsion of the fœtus alone, living c the term or otherwise, the pregnancy being simple or of Five cases of success of expulsion of the placenta. iii. Fi success in flooding after delivery. iv. Sixteen cases of success, which consisted of cases in which the ergot exci pulsive powers for a certain time only, the delivery not b nated naturally till several hours after the employm medicine, or of cases in which, after having advanced th a certain degree, the application of instruments becan and was made. v. Eighty-two cases of complete failur the ergot had no sensible effect, producing no retur action, whatever doses were given. vi. Twelve disagree results, either for the mother or child, attributed by diffe to the immediate action, or to the secondary effects c This proportion of seven and a half of success to one seldom furnished by other therapeutical agents employe any morbid state ‡.

7. On the Holotures and particularly of the physalis.—(*Linn.*)

This species of molluscæ, vulgarly *galère* in French,

* Revue Ency. xlix. 239. † Recueil Industriel
‡ Med. Phys. Journal, 1831, 462.

lish, 'Portuguese man-of-war,' so rare in collections, so difficult to preserve, so incompletely described by naturalists, and, it must be owned, so little worthy of observation when deprived of life, is, perhaps, one of the most curious inhabitants of the equatorial seas. There are few navigators who have not sought to ascertain some of the habits of life of these singular animals, whose extraordinary form, brilliant colours, and habit of remaining floating on the surface of the water during calm weather, has attracted the attention of all navigators. These habits are the origin of the vulgar names given to them by the sailors. The body of the smallest of these creatures which we have been able to observe, was about 2 centimètres (0.8 of inch) long, and that of the largest was 17 centimètres (6.7 inches). Their form, which it is impossible to compare to that of any other animal living, rather resembles a small bladder stretched and filled with air, of an azure blue, slightly streaked with deeper tints and green; their body, almost cylindrical, is surmounted by a crest, which is in plaits, very moveable, and edged by the most lively tints of purple and rose-colour. This little crest serves the animal for a sail, and by the disposition which it gives it, regulates its movement in nearly the same manner as a ship. According to the strength of the wind, it spreads, rests, or compresses its sail, and in heavy weather, it allows itself to float, by means of a respiratory apparatus of a peculiar construction. The lightness of its body is such, that it appears resting on the water, and when plunged in alcohol it floats again to the surface of that fluid. The lower and middle part of the animal is armed, at different lengths, with tubes, papillæ, and retractile feelers, some of which are from sixteen to eighteen feet long, disposed spirally or in chaplets of the most beautiful blue, and most delicate rose-colour, and serve at once as organs of absorption, defence, and locomotion. These tubes, papillæ, and fibres, contain a viscous matter, which produces pustules on the human skin, and occasions a pain similar to that of a large but superficial burn. This property is not easily got rid of; vessels in which one of these animals has been plunged, must be washed several times in water, and carefully scoured before they can be used without inconvenience; and linen, which had merely been rinsed in soap and water, had this quality of irritation fifteen days after it had been used in making observations on these animals. Cutting these feelers does not produce death, at least for a considerable time; and incisions made transversely in the body with scissors do not deprive the animal of life. The membranous crest appears to have more irritability than the other appendages, and the animal appears to contract itself, and to suffer more when tormented there than in any other part. Naturalists suppose that the *holothurus* feeds on animals of all kinds, occasionally on some of a very considerable relative size, and that they have a very strong and active digestion. They, in their turn, serve as food to species of the scombri and *medusæ*, against which their weapons of defence are unavailing

8. Defence against Flies, used by the Butchers of Ge

It is said that the butchers of Geneva have for a long time u oil of laurel as a substance which prevents the flies from ap| ing their meat. The odour of this oil, though strong, is n disagreeable, and the flies will not approach the walls or part have been rubbed with it. The person who describes these says, that he has, in this way, guarded the gilt frames of and pictures most perfectly from flies *.

9. The Palm of Chile.

It is chiefly in the middle province that the palm of Chile (*Micr* is found. It is not a common tree, being very partially dis| but several of the estates owe much of their value to the nu *palms* upon them; and, although the stem is useless, the sap, and fruit, yield a large income to the proprietor. For t| houses, the leaves are considered better and more durable | other material; the sap, boiled down to a syrup, is used a stitute for honey, and has a very agreeable flavour; and t cocoa-nuts, about an inch in diameter, of which every tree | a great number, are highly esteemed, and form a con article of export to Peru. A curious method is employed the nut from the green husk in which it is enveloped, a that was formerly attended with a great loss of time an| A number of cows and oxen are driven into an enclosure quantity of the fruit is spread, and being very fond of its h immediately begin to feed on the fruit, only slightly mast in the first instance, and swallowing the whole; afterwa| chewing the cud, the nuts are rejected; and when th finished, a heap of them is found before each of the anir fectly free from the husk, the cattle being thus supplied at a season when little grass remains on the hills, at the | that they effectually perform a very useful operation †.

10. Destruction of Weeds in Paved Paths and C

The growth of weeds between the stones of a paveme| very injurious as well as unsightly. The following adopted at the Mint at Paris and elsewhere with good ef hundred pounds of water, twenty pounds of quick lim| pounds of flowers of sulphur, are to be boiled in an iron \ liquor is to be allowed to settle, the clear part drawn off, more or less diluted, according to circumstances, is to b watering the alleys and pavements. The weeds will not several years ‡.

11. Preservation of Hay.

Eye-witnesses assert that in Russia the inhabitants usua

* Recueil Industrielle, xv. 247. † Botanical Miscellany

‡ Recueil Industrielle, xv. 246.

hay with all its natural verdure. To obtain this effect, the grass, as soon as cut, is (without being allowed to fade) instantly stacked. A kind of chimney, made with four rough boards, is constructed in the middle of the stack, and it appears that this channel prevents the accumulation of heat from fermentation; and that the herb thus treated retains all its leaves, its colour, and its primitive taste. The size of the stacks is not mentioned *.

12. Remarkable Propagation of Wind.

Whilst the bells were ringing to church, at Albany, on the 12th of July, 1829, a very violent gust of wind from the south-east passed over the town. This gust passed over New York, which is to the south of Albany, when the service had proceeded for some time: so that this south wind was rendered evident in the northern town an hour nearly before it was felt at the southern position, and it had been propagated from north to south in the direction exactly contrary to that in which it blew.

Franklin remarked that violent north-west winds in the United States frequently had their origin in the quarter towards which they passed, and was inclined to attribute them to great and sudden alterations in the atmosphere of the Gulf of Mexico. To explain the present instance in the same manner, a diminution in the atmospheric pressure to the north of Albany must be considered as having occurred †.

13. Thoughts on North and South Winds.—(*By M. Alphonse Blanc.*)

It has been generally admitted that winds are mostly, if not always, caused by dilatations or condensations of the air due to changes of temperature. In fine, quiet weather, the wind from the east in the morning, often becomes south at midday, and west in the evening, and may, with great appearance of reason, be attributed to the dilatation of the air by the sun in the east, south, and west in succession, of the place of observation. It is also known that a wind often blows in one place before it blows in another place to windward of the first; and in such cases it has been supposed that the effect is due to some great condensation in the air at some place to leeward of the place where the wind is felt.

Now the barometer should be affected differently according to the nature of the cause of wind. If condensation occurred about the pole, the air of surrounding places should flow towards it, a partial vacuum should occur in those places, forming a south wind gradually extending towards the south, and the others to which the wind should reach, and the barometer should fall at those places. But if expansion of the northern air occurred, a north wind should commence at the north, and be propagated to the south, driving or accumulating the air before it, and the barometer should rise in all those places to which it reached.

* Recueil Indust., xv. 247.

† Ann. de Chimie, xlv. 420.

The effects due to air passing from the north southwards
be greater, because, arriving at warmer regions, it would t
heated, and tend to dilate; the contrary effect would occur w
passing from the south to the north. But these effects wo
smaller, if the changes of tension which act as causes of the
occurred to the south instead of in the north, because the air,
forward by expansion, would contract as it travelled north
and *vice versâ*.

The variations of the barometer between the tropics is scarc
thing; the winds are very regular, the cause is permanent, a
equilibrium appears constant. From this regularity, it may b
cluded, that the north and south winds which we have, origi
the north; for how can it be imagined that a south wind sho
nerally come to us from a place where the contrary wind is co
On this view, the south wind would generally be caused by
densation, and the north wind by an expansion of the polar
phere, and the barometer ought to fall with south winds and r
north winds. This effect is usually observed.

Though the south wind, in passing northward, is cooled,
posits part of its vapour in clouds and rain, it would appear, f
above reasoning, that such deposition is not the cause of the
of the barometer. If vapour were the cause, the effec
to occur in every place where there is rain; but this does
pen. It may easily be conceived, that although the south w
ordinarily be caused by condensation of air in the north, an
companied by depression of the barometer, it may sometime
to dilatation in the south, and then the barometer would rise

14. Nitrous Atmosphere of Tirhoot.

Tirhoot is one of the principal districts in India for the ma
of saltpetre: the soil is every where abundantly impregn
this substance, and it floats in the atmosphere in such quant
during the rains and cold weather it is attracted from them
lime on the damp walls of houses, and fixes there in shap
downy crystals of exceeding delicacy. From damp spo
be brushed off every two or three days almost in basketsful.
sequence of all this, the ground, even in hot weather, is so
it is extremely difficult either to get earth of sufficient t
make bricks, (the country being quite destitute of stones,
made, to find a spot sufficiently solid to sustain the weight
Even with the greatest care the ground at last yields, an
petre corrodes the best of the bricks to such a degree, that
house gradually sinks several inches below its original lev
built of inferior materials of course suffer much more; on
the inner foundations were of unburnt bricks, absolutely
whilst I was at Mullye, and the family in it escaped
miracle. My own house, which was not much better, san

* Annales de Chimie, xlv. 421.

and the walls at bottom were so evidently giving way, that I was compelled, with extreme expense and inconvenience, to pull down the whole inner walls and build them afresh, in a more secure manner. From the same cause a new magazine, which government directed to be built with an arched roof of brick-work, was, when complete, found so very unsafe, that it was necessary to demolish it entirely and rebuild it on a new plan, with a roof of tiles*.

15. Progressive Motion of the Glaciers.

The ladder which M. de Saussure used in crossing the crevices in the ice during his first visit to the Col du Géant, and which he left on the upper part of the glacier, has lately been discovered imbedded in the Mer de Glace, in a situation nearly opposite to the aiguille called Le Moine. This ladder, moving on with the body of ice, will thus appear to have advanced three leagues since the year 1787.

Captain Sherwill, in his relation of his ascent of Mont Blanc, speaking of the glaciers, says, 'In traversing these stagnated oceans, very large blocks of granite, of many tons weight, may be seen riding on the surface of the ice. These blocks have afforded the means of ascertaining a fact of importance. The experiment I am about to relate to you was made last year by some of the guides of Chamouni. Two poles were erected, one on each side of the glacier, out of reach of its movement, and so placed as to be in a direct line with a block of granite. In the course of twelve months, this block had entirely changed its position, as respecting the two poles, and had advanced about one hundred yards on its march towards the valley, a clear proof that the glaciers do move on, and are continually diminishing at their lower extremity, by the melting of the ice, and increasing at the upper end by the constant snows.'

If the progress made by the ladder of M. de Saussure taken for one year, and the result of the experiment made at the instigation of Captain Sherwill, should not appear to agree, it must be recollected that from the Col du Géant to the spot where the ladder is, is a very rapid descent, and of course the march of the glacier would be rapid in proportion; whereas the experiment of Captain Sherwill was made on a level part of the same glacier, the Mer de Glace, where the ice is of a more compact texture than that at an elevation of above ten thousand feet, and consequently its progress towards its final issue would be somewhat slower †.

16. On the Comparative Quantity of Salt contained in the Waters of the Ocean.—(*By M. E. Lenz.*)

The following experiments were made on the expedition of Captain Kotzebue:—Those with water at seven different places of the ocean, and at various depths, tended to prove, that from the equator to

* Calcutta Transactions. † Philosophical Magazine, N. S. ix. 32.

45° north latitude, the ocean contains, at the same place, the san quantity of salt, as low at least as 1000 toises. For ascertaining t quantity of salt contained in the water on the surface of the oce the observers used hydrometers which, by laying weights on the were made to sink into the water to a mark on the thin neck of t instrument. It is well known that Dr. Erman has ascertained contraction of salt water of the specific gravity 1·027 for every gree of the thermometer, from 12 R. (59 F.) to -3° R. (25¼ I and has proved that such water has no maximum of density, bef freezing, like common water. With a view to the investigation l under consideration, the table of Erman was extended, by exp ments made for the purpose, as far as to 24° R. (86 F.) It then ascertained that a small difference in the specific gravit the water had no great influence on the law of contraction by ch of temperature, since experiments instituted with this view pr that two portions of water, whose specific gravity was 1·02 1·03, (between which limits undoubtedly all waters of the ocea comprehended,) observed nearly the same law, and perfectly s those temperatures at which observations are made on the o The experiments could be very approximately represented b expression for the density (d) of water whose specific gr = 1·027, and whose temperature is (t) degrees of Reau thermometer:—

$$d = 1 - 0{\cdot}0002053.\ t + 0{\cdot}0000003723.\ t^2 - 0.000000188086$$

and by this formula the numerous observations of specific gra water, in different parts of the ocean, were reduced to the temperature, viz. 14° R. (63½ F.)

The general result of all the observations is as follows:—

i. The Atlantic Ocean contains more salt than the Ocean, and the Indian Ocean is, therefore, more salt ne Atlantic than near the Pacific.

ii. In each of these great oceans there is a northern southern maximum of salt; the northern is farther from the than the southern. The minimum is, in the Atlantic, a few north of the equator; the same is probably the case in the there were, however, not sufficient observations to prove it.

iii. In the Atlantic Ocean the water is more salt to the we there seems to be no such difference in the Pacific.

iv. The greatest specific gravity of the water in the Ocean, at the northern maximum, in lat. 20½° and long. 4 Greenwich, was found to be = 1·02856; that of the Pacifi southern maximum, in lat. 17° and long. 119° W. = 1·

v. To the northward of the northern maximum, and to the s of the southern maximum, the specific gravity of the wat nually decreases with the increase of latitude.

It is clear that the quantity of salt in the water on the s the ocean must mainly depend on the evaporation; and as

pends again on the temperature of the air and on the wind, it will be found that the different degrees in which these causes operate together, will afford a satisfactory explanation of the circumstances here described *.

17. Hourly Observations of the Barometer in the Fortress of Cavite.

These observations were made by the scientific men attached to Captain Kotzebue's expedition in the year 1823—1826, on the Island of Luzon (14° 34′ north lat., and 239°9′ west of Greenwich) for ascertaining the periods of its regular falling and rising during twenty-four hours. The barometer was kept in a room six toises above the level of the sea, in which the temperature was nearly the same day and night, (about 25° Centigrade, or 77 Fahr.) and the observations were made on eleven different days between the 12th and 26th of December. The following are the general results deduced from the whole of the observations. The barometer has a maximum of

height at . .	9^h	1′	1″	A. M.	
it then falls till .	4	28	6	P. M. on an average	1.04 line;
it rises again till .	9	58	3	P. M. „	0.687 „
and falls again till .	4	30	0	A. M.	——
lastly it rises again till	9	1	1	A. M. on an average	0.445——

18. Comparison of the Mean Temperature of the Air and the Water on the Surface of the Ocean within Twenty-four Hours.

These observations were made on the expedition commanded by Captain Kotzebue during the years 1823—1826. Four observations were made within twenty-four hours; one half an hour before sunrise, the second an hour and a half after noon, and the two others at equal intervals between the afternoon's and next morning's observations. The result which is obtained from all the observations is this: in the zone from 45° N. lat., to 30° S. lat., the mean temperature of the water on the surface of the ocean, whether taken for the whole year, for single months, or for single days, exceeds the corresponding mean temperature of the air with which it is in contact. Beyond these latitudes, the means taken for twenty-four hours, during the summer months, (there being no observations during the winter months,) varied, sometimes the one, sometimes the other being higher.

The following were the highest mean temperatures for twenty-four hours:—

In the northern hemisphere, of the air=27°.225 C (81°.0 F.); of the water 27°.55 (81°.6 F.)

In the southern hemisphere, of the air=26°.975 C (80.53 F.); of the water 28°.30 (82°.9 F.) †.

* Petersburgh Transactions, 1830. † Ibid.

19. Professor Oltmanns on the Geography of South America *.

It is well known that the results of the expedition of Malesp which was most richly fitted out for scientific investigations, and r ably conducted, have never been communicated to the public. Bauza, a skilful officer attached to the expedition, communicate Professor Oltmanns the astronomical observations made on tha pedition, which he has saved from the fate of the other fruits of ill-starred expedition, on condition that he would recalculate th Professor Oltmanns has redeemed his pledge, and in laying be the world the results of his calculations of the observation Malespina, together with those of other observers, he confi the opinion expressed by Bauza, that the former calculations very inaccurately executed. He likewise agrees with M. B respecting the inaccuracy of the observations made on boarc Conway, some of which, when recalculated by Professor Oltma gave results differing more than twenty minutes in arc from wh most probably true. From the numerous list given by Profe Oltmanns, we extract the following positions of some remark places on the coast of South America, as corrected by Profe Oltmanns; among which the longitude of Rio Janeiro dep on a great number of occultations of Jupiter's satellites.

Places.	Latitude.			Longitude west of		
Rio di Janeiro	22°	54′		45°	36′	[illegible]
Buenos Ayres . . .	34	36	40″	60	47	
Monte Video (Observatory) .	34	54	40	58	36	[illegible]
San Carlos de Chiloe .	41	52	0	76	11	[illegible]
La Concepcion . . .	36	49	30	75	25	[illegible]
Valparaiso . . .	33	2	0	74	2	[illegible]
San Jago de Chili . .	33	26	15	73	17	[illegible]
Coquimbo . . .	29	56	40	73	43	[illegible]
Callao (Casillo del Callao) .	12	3	40	79	34	[illegible]
Guayaquil . .	2	12	0	82	18	[illegible]

20. On Beauchamp's Geographical Positions in the Eas
(By Professor Oltmanns.)†

It is well known that the geography of the interior of Asia was fectly unknown till within about half a century, and even at moment there are, in the interior of Persia, and other central reg of Asia, large tracts without a single place whose position is d mined by astronomical observations. We are indebted to travellers, Niebuhr and Beauchamp, for the first advances ma the geography of the south-western parts of Asia. Niebuhr's

* Berlin Academy, Year 1827: Berlin, 1830, 2d Part, p. 37.
† Mem. of Berlin Academy, Year 1827. Berlin, 1830, p. 139.

servations were in that part, however, confined to latitudes, while the lunar distances observed by him in Egypt have, after a lapse of fifty years, been calculated by Bürg, and received by the learned with the praise which they deserve. Beauchamp made two scientific journeys to the East, but was much better provided for making observations on the second, than on the first. The published observations of this rather capricious traveller did not answer to the high expectations that had been formed. The numerous errors, especially in writing, have baffled the exertions of some of the ablest and most indefatigable calculators; and the suspicion that some of his observations have been subsequently corrected by himself, as well as the circumstance that his observations at Casvin of a lunar eclipse did not give the position of the Caspian Sea, near which that place is situated, have lowered the estimation of Beauchamp's labours. Professor Oltmanns obtained a manuscript of some of Beauchamp's observations which was supposed to be lost, entitled 'Relation Historique et Géographique d'un Voyage de Constantinople par mer, l'an V de la République. Par le Chevalier Beauchamp. 38 pp. fol.' In this he was likewise much disappointed, as, of many observations, results only were given; while in others it appeared that Beauchamp always neglected the aberration and nutation. Notwithstanding this, Professor Oltmanns has thought it worth while to recalculate the observations by the latest tables, and he has thus determined the following geographical positions:

Trapezunt.—Lat. (mean of two observations) 41° 2′ 18″.
Long. (by chronometers, lunar distances and eclipses of Jupiter's satellites, all agreeing tolerably well), mean 37° 11′ 18″ E. of Paris.

Sinope.—Lat. (mean of seven observations of the sun and stars) 42° 1′ 42″.
Long. (mean of chronometers and eclipses of Jupiter's satellites) 32° 45′ 33″ E. of Paris.

Gydron.—Lat. (one star observed) 41° 54′ 6″.
Long. 3° 37′ E. of Constantinople.

Jeniki.—Lat. by three observations of the sun, mean 41° 59′ 49″.
Long. by chronometers, 31° 16′ 15″.

Amasra.—Lat. (according to Beauchamp's own calculation), 41° 46′ 8″.
Long. 29° 50′ 30″.

Neracle du Pont or Eregri.—Lat. by four stars, mean 41° 17′ 6″.
Long. by two eclipses of Jupiter's satellites, mean 29° 9′ 56″ E. of Paris.

Bagdad.—Here Beauchamp had an observatory on his first expedition: lat. then found, 33° 19′ 50″.
Long. by eclipses of Jupiter's satellites, a solar eclipse, and an occultation of Jupiter by the moon, all nearly agreeing, mean, 42° 2′ 5″ E. of Paris.

21. Eumenian Marbles.

A discovery of great historical importance has lately been made Autun, a city about 160 miles to the south-east of Paris. It w well known that Eumenes had placed in the schools which bore h name a marble tablet representing the itinerary of the Roman roa leading from the territory of the Ædui in Celtic Gaul, to Ital This marble, having been accidentally broken, was employed, common with a number of other ruins of ancient monuments, forming the foundation of the Abbey of St. Jean-le-Grand, found at Autun at the close of the sixth century by Brunehaut. Th precious marble was considered as irretrievably lost; but M. Ma tigny, of Autun, has undertaken an excavation, by means of which has already recovered a fragment of the itinerary, a marble ewer, capital, &c. Should M. Martigny succeed in recovering the wh of the marble, it will probably be of great use in correcting completing the Itinerary of Antoninus, the table of Peutinger, a the Arundel marbles. The Academy of Sciences of Dijon has t fragment which has been found, and is preparing to have it engrav and published.

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22. On the Illumination of Theatres.—(*Additional Remar by Mr. Ainger.*)

Since the paper printed at page 45 was written, I accidenta learned that an essay on the same subject had been published M. Lavoisin, in the Memoirs of the French Academy for 1781. have examined the essay in question, and I find, as might ha been expected, that the objections to the existing mode of illumin tion had been fully felt and expressed half a century since. T plan suggested by M. Lavoisin is, however, very different from th which I have submitted. In the first place, M. Lavoisin retai the foot-lights, in which are, I think, comprised nine-tenths of t objections to the present system; preserving these, it seems scarce worth while to incur any considerable expense to remove the oth comparatively trifling evils. M. Lavoisin, in addition to the foo lights, suggests the use of certain powerful lamps with reflect above the stage for the purpose of illuminating the centre scene wh it is so far back as to be ineffectually lighted by the foot-lam He proposes to light the audience by nine burners, with ellipsoi reflectors, placed above the ceiling, which is to be perforated for t purpose; but as this would leave the ceiling itself quite dark, adds other lamps round the walls, with a view to remove th defect.

It is gratifying to find that the subject has been thought s ficiently important to merit the attention of so distinguished a p losopher as M. Lavoisin; and on his authority I venture to ho that it will be taken up by those who alone have the means making an effectual experiment.

Vol 2 Plate 2

A Drawing of
THE HEAD OF BOLA.
A NEW ZEALAND CHIEF.
illustrate "An Account of the Mode of preparing Human Heads in New Zealand &c."
By George Bennett, Esq. M.R.C.S. &c

Drawn from the original Head by M A H. On Stone by M Gauci

London, Published by John Murray, August 1st 1831.

Plate 5th.

Fig. 1.

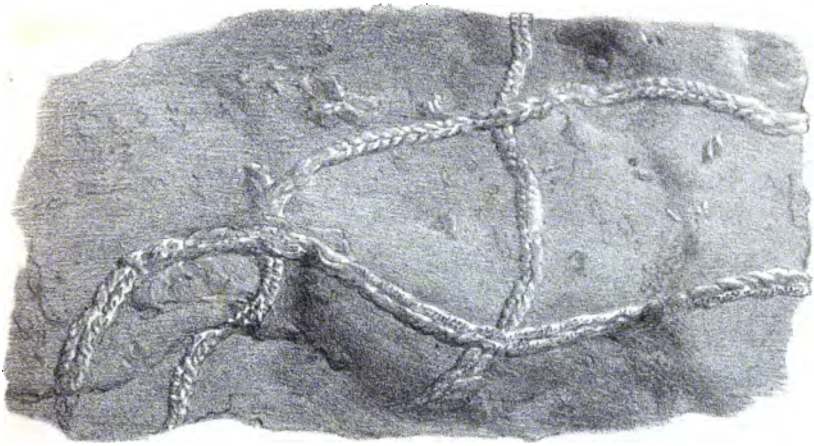
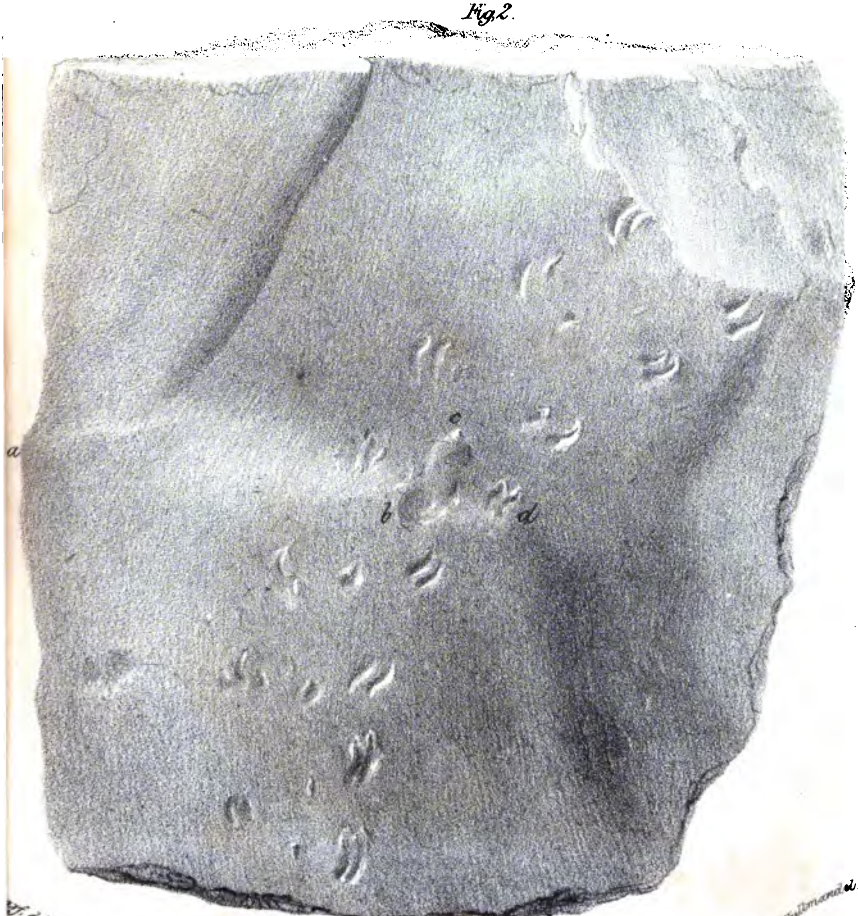


Fig. 2.



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London Published by John Murray April 1831

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On the Illumination of Theatres.

Fig. 1

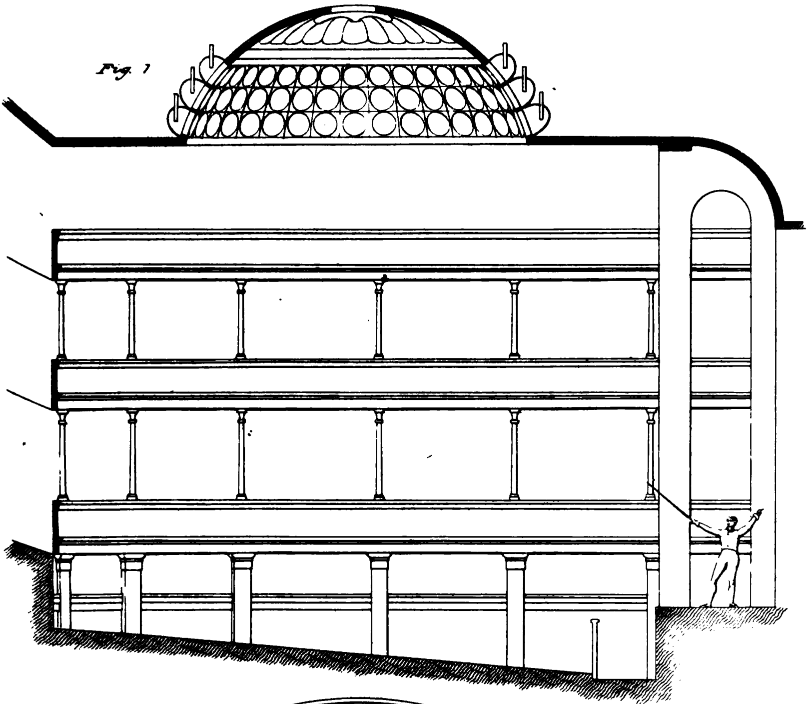
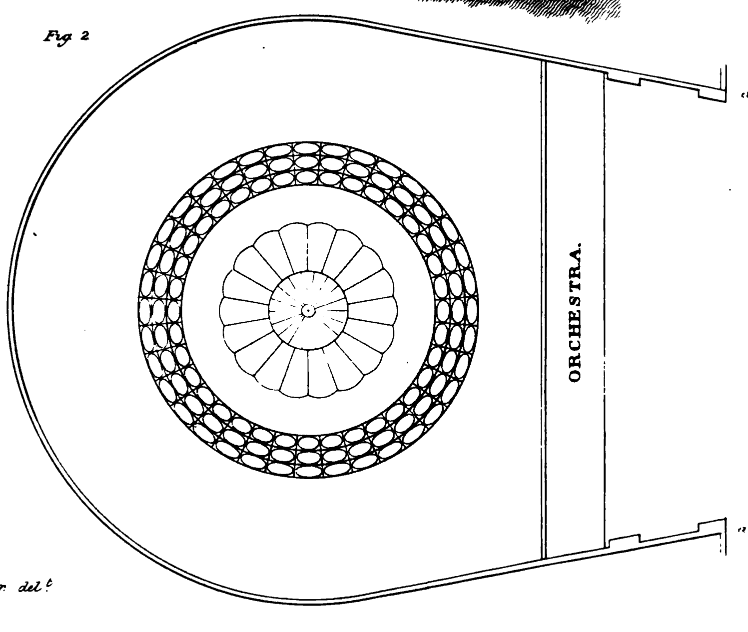


Fig. 2



J. Ainger. delt.

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OF

THE ROYAL INSTITUTION

OF

GREAT BRITAIN.

THE MODE OF PREPARING HUMAN HEADS AMONG THE NEW ZEALANDERS, WITH SOME OBSERVATIONS ON CANNIBALISM*.

By GEORGE BENNETT,

Member of the Royal College of Surgeons in London, &c. &c.

IT is now fully ascertained, that the natives of the New Hebrides, the Marquesas, New Zealand, and other Polynesian islands, are cannibals; yet it is among the New Zealanders only that the custom obtains of preserving the heads of their enemies as tokens of victory, and as objects of contempt. There is something analogous to this among certain tribes of Africans, who preserve the skulls of their enemies for purposes similar to those of the New Zealanders. Captain Tuckey observes, respecting this custom among the natives on the river Zaire or Congo, 'the first objects that called our attention were four human skulls, hung to the tree, which we were told were *those of enemies' chiefs taken in battle, whose heads it was the custom to preserve as trophies;* these victims, however, seemed to have received the *coup de grace* previous to the separation of the head, all the skulls presenting compound fractures.'—page 101. The New Zealanders sometimes, however, preserve the heads of their friends, but for very different purposes—those of paying respect to the memory of the deceased; to show to their relations who have been

* The plate was given in the last Number.

absent at the time of their death; and that lamentations may be renewed over them at certain periods.

The preparation of the Moko, or head, among the New Zealanders, most effectually prevents decay, and is at th same time compatible with the perfect preservation of the fea tures. The process, as practised among them, is as follows:– After the head has been severed from the body, the base the skull is broken by a stick or stone, and the brain remove &c.; the cavity of the cranium is then frequently washed o until it is well cleansed. The head is then dipped for a fe minutes in hot water, which causes the cuticle to peel off; th are careful at this time not to touch the hair, as by so doing would readily come off, but it afterwards, when cool, becom more firmly fixed than before, as is well exemplified in t specimens brought to this country. A piece of thin stick next placed up the septum narium, for the purpose of keep the nose in shape, aided also by another piece placed interna to bear against the nose as a prevention against its sinking; nostrils are also stuffed with muka or flax, or pieces of wood, for a similar purpose; the eyes are taken out (if th of a chief they are eaten, if otherwise they are thrown aw and the eyelids are stitched up, as well as the mouth, for purpose of preserving their shape. A pit having been viously dug, hot stones are brought and placed in it: this which is indeed the usual native oven, is so constructed, it is covered over, with the exception of an aperture abov sufficient size to permit the base of the head to rest upo Water having been previously thrown over the hot stones, repeated as often as considered necessary, a steam is duced, which is also increased by the addition of leaves tened with water; the base of the head is placed ove aperture, and the heat and steam ascend into it; th stones, water, &c., to keep up the requisite heat and during the process, being renewed as often as required person in attendance, until the preparation is completed. person whose duty it is to attend to the preparation of th is careful, when any of the skin of the face appears to w to smooth down and preserve it in shape. The time oc in this process previous to its completion is from twen

to thirty hours. After the head is prepared, it is removed from the oven, placed upon a stick in the sun, and is frequently oiled, but this latter process is not considered necessary for its preservation, but is intended only to give it a more finished appearance. The adoption of this simple, but at the same time excellent method of preserving the human heads, would enable a valuable series to be formed, illustrative of the varieties of the human race.

The object of the natives in thus preparing the heads of their enemies, is to preserve them as trophies, as well as for the gratification of their revengeful feelings; they exhibit them in their war-dances, holding them up as objects of contempt and ridicule; and when advancing to battle, they display them before the hostile party, and accompany the exhibition by arrogant and insulting speeches. They generally consider these heads as tokens of victory; they are brought home by the conquerors to their wives and children, that they also may have an opportunity of rejoicing with them over their fallen enemies, and they offer them to the spirits as a thanksgiving for victory. At the Bay of Islands, Hookianja, North Cape, &c., the chiefs who die (with but few exceptions) are buried unmutilated; but at the Thames, East Cape, &c., the heads of the chiefs are generally preserved, both out of respect for the deceased, and to show to those of his relations who are absent at the time of his death. These heads are never sold, the heads of their enemies only being thus disposed of as a mark of contempt towards them.

One of the heads in my possession (and of which a drawing is annexed) I purchased at the river Thames, and, what is rather unusual, was able in this instance to procure the name, rank, character, &c. of the individual, and it was procured from the chief by whom he had been slain. He was named Bola; his father's name was Tumau, and he was a young chief of the district of Wigato, at the river Thames. His age was supposed to be about eighteen years, and he had not long been tattooed, the whole of which process was not completed at the time of his death. He was described as being a great warrior for his age, and a very enterprising character, always endeavouring to be the first in battle, and to kill the first man,

which exploits are considered among them as highly honourable. During an engagement, he was first shot in the abdomen by a chief named Warrinhu Eringa (who related to me some of this account), and, on falling, was finally dispatched by a blow on the head with a tomahawk; on an examination of the side of the head, the fracture is very visible, and is of some extent.

The New Zealanders care not to conceal that they are cannibals; they relate the atrocities connected with the practice without any appearance of shame or remorse; they only eat however, of the bodies of their enemies. If an enemy of ran is slain, the eyes, hands, and feet are presented to the highes chief of the conquering party, as they observe, that 'with th eyes their enemy saw his adversaries, with his hands he fough and with his feet he invaded their territory—they bore him the combat.' There was a chief of a district in the vicinity the river Thames, who was pointed out to me as the one wh killed the noted chief named Atoi, or Pomari, and who w said, in their style of expression, to have 'eaten of his eye and drank of his blood.' Respecting the eating of the ey there formerly existed a somewhat similar custom at the isla of Tahiti, from which it has been inferred, that the natives that island were formerly addicted to the horrible custom cannibalism: the coincidence is curious, and Captain Co makes the following observations respecting it. 'We h great reason to believe,' he observes, 'that there was a t when they were cannibals. We were told (and, indeed, pa saw it) that it is a necessary ceremony when a poor wretch sacrificed, for the priest to take out the left eye; this he pres to the king, holding it to his mouth, which he desires hir open; but instead of putting it in, immediately withdraw This they call "eating the man," or "food for the chief;" perhaps, we may observe here some traces of former ti when the dead body was really feasted on.' Ellis obse however, in a more recent publication (*Polynesian searches*, vol. i. pp. 35, 357,8), that 'It has been supp that the circumstance of the priests offering the eye most precious part of the victim, to the king, who appear eat it, indicated their having formerly devoured the men

had sacrificed.' 'I do not,' he further observes, 'regard this fact as affording any very strong evidence, although I have not the least doubt that the inhabitants of several of the South-Sea Islands have eaten human flesh. From the many favourable traits in their character, we have been unwilling to believe they had ever been cannibals; the conviction of our mistake has, however, been impressed by evidence so various and multiplied as to preclude uncertainty. Their mythology led them to suppose that the spirits of the dead are eaten by the gods or demons, and that the spiritual part of their sacrifices is eaten by the spirit of the idol before whom it is presented. Birds resorting to the temple were said to feed upon the bodies of the human sacrifices, and it was imagined the god approached the temple in the bird, and thus devoured the victims placed upon the altar. In some of the islands, "man-eater" was an epithet of the principal deities; and it was probable, in connexion with this, that the king, who often personated the god, appeared to eat the human eye.' Notwithstanding these judicious remarks, the coincidence is very extraordinary with the custom of New Zealand, where the eye is actually devoured, and where the natives are well known to be cannibals; and what further corroborates the supposition that the Tahitans were anciently anthropophagi is, that, as the author before quoted observes, (*Polynesian Researches*, vol. i., p. 310,) 'the Tahitans were not altogether free from cannibalism; and occasionally a warrior, out of *bravado or revenge*, has been known to eat two or three mouthfuls of a vanquished foe, generally the fat from the inner side of the ribs.' From this it would appear, that the exciting cause of cannibalism is, both with this people and the New Zealanders, revenge: for cannibalism, the New Zealanders informed me, arises from this feeling, not from hunger; and from believing that, by eating of the bodies of the valiant, as all those are considered who die fighting on the field of battle, they become inheritors of their courage and valour. The horrible practice of cannibalism having been found existing in the most fertile countries, we must seek for some other motives for the custom than mere hunger; and the above causes, as asserted by the natives, seem to be the most probable. To

eat the bodies, however, hunger would be requisite, combi with the revengeful feeling: all provision being sent away f the field of battle, with the women and children, hui becomes a concomitant with the revengeful feeling, but the sole exciting cause. After a battle, it is customar collect all the dead bodies of the enemy together, and heads of those intended for preparation having been detac are delivered to the persons who are habituated to it; bodies are then cut open, the viscera, &c. extracted, anc remainder cut into pieces; they then proceed to cook prepare the banquet—in what manner they are not partici some make an oven and steam it, others roast on the fire they seldom or never eat it in a raw state; but it is a com and general custom, on an enemy falling in battle, fo adversary, excited by the demoniacal spirit of revenge, to immediately towards him for the purpose of sucking the l from his throat, before the vital spark has fled. They dry the hands of their enemies, and fasten them near huts, the fingers having been previously dried in a contr form, so as to be used as hooks, on which to hang their ba &c. They also preserve the fat from the buttocks, the internal fat or fare; they melt it down by the aid c stones, keep it in calabashes, and eat it with their pota this is more generally done when the person is a po chief, and they always express it as being a mark of contempt towards him. On my asking some of those gave me information respecting this horrible custom, hov would like to be eaten, the reply was, 'that it was no i what was done with them after death.' On my inquiring was done with the bones of the human bodies that were I was informed that, if those of a chief, they were pres those of the arms, legs, &c. being used for making the named Lehu or Bulrua, others as ear ornaments, &c.; b bones of a common individual were thrown away. respect to the taste of this food, they describe it as superior to pork. Vessels are occasionally destroyed natives, and the crews massacred; at one time several of unfortunate Europeans, who had thus been murdere served in a similar manner as among themselves, wer

chased and brought by a colonial vessel to Sydney, New South Wales. One of the chiefs at the river Thames, from whom I made the inquiry, whether he had ever eaten of the flesh of white men, and whether it was better tasted than that of a New Zealander, replied, that 'he had tasted of the flesh of Europeans: sometimes he found it good, sometimes bad, but generally very salt.' It is a curious circumstance that the natives of New Zealand always express a dislike to salt. It is customary, if a chief is ill, for a slave to be killed, as an offering to the spirits, but the body is not eaten; but if a chief is slain, or deeply offended by the chief of any particular district, and his relations should have any slaves in their possession belonging to that district, they are killed and eaten from revenge. During my visit to New Zealand, in June 1829, I was rambling on shore at Wyshaki Cove, River Thames, on a botanical excursion, when, among some rushes which grew on the borders of a rivulet, I observed some bones protruding, and, on a closer examination, found a heap of human bones, apparently belonging to one person. I thought there had been a cannibal banquet at this place, and I brought away several of them with me; but on showing them to a chief, he said they were those of a person who died a natural death; had they been those of a person who had been killed and eaten, they would not be in so perfect a state; and on mentioning that I had found them collected together in a heap, confirmed him in his opinion. He also said of the lower jaw, that if it had been that of an enemy, it would have been cut down, and used as a fish-hook (matau).

At the village of Kororadeka, Bay of Islands, which is much frequented by whalers and other vessels for refreshments, and which is situated opposite to the missionary station of Paihia, several cannibal banquets have taken place on the beach.

Some of the notions which persons in this country entertain on the subject of cannibalism are very erroneous; since my arrival in England, I have had several curious questions asked me: among numerous others this was one—Whether a child, which I brought from Erromanga, one of the New Hebrides group (where they are cannibals), could eat our food? Surprised at the question, I asked why not? 'Because,' was the

reply, 'I thought that, after having been accustomed to de human flesh, she would not be able to relish any other kin

It is supposed that, by purchasing the preserved heads i the New Zealanders, an encouragement is held out for t to engage in war, or to murder their slaves. This I con erroneous. They have preserved them, from time immemc as trophies, and whether they are or are not purchase Europeans, the custom will continue, until civilization extended among these noble, but savage people. Duri long stay at New Zealand (and that principally at the Thames, where it is generally considered they might be chased in some quantity), not more than six were procu the reason assigned for their scarcity by the natives being, there had been lately no wars among them.

In conclusion, we may observe, with Dr. Good, that common character runs through savages of every kind. empire of the heart is divided between two rival deitie rather demons—selfishness and terror. The chief min of the first, are lust, hatred, and revenge—the chief min of the second, are cruelty, credulity, and superstition. through the world, and you will find this description app barbarians of every age and country. It is equally the h of Europeans and Africans; of the Pelasgi, who wer progenitors of the Greeks; and of the Celts and Scyth the successive progenitors of the English. All the disco of modern circumnavigators confirm the assertion; and th the captivating names of Friendly and Society Islands been given to two distinct groups in the vast bosom c Pacific Ocean, and the inhabitants in several of them made some progress in the first rudiments of civilizatio government, there is not a people or a tribe to be met who are yet in a savage state, that are not still slaves to debasing and tyrannical passions*.'

* Book of Nature, vol. iii., p. 280-81.

ON THE TRANSMISSION OF MUSICAL SOUNDS THROUGH SOLID LINEAR CONDUCTORS, AND ON THEIR SUBSEQUENT RECIPROCATION.

By CHARLES WHEATSTONE.

§ 1.

THE fact of the transmission of sound through solid bodies, as when a stick or a metal rod is placed at one extremity to the ear, and is struck or scratched at the other end, did not escape the observation of the ancient philosophers: but it was for a long time erroneously supposed, that an aëriform medium was alone capable of receiving sonorous impressions; and in conformity with this opinion, Lord Bacon, when noticing this experiment, assumes that the sound is propagated by spirits contained within the pores of the body*. The first correct observations on this subject appear to have been made by Dr. Hooke in 1667; who made an experiment with a distended wire of sufficient length to observe that the same sound was propagated far swifter through the wire than through the air †. Professor Wunsch, of Berlin, made, in 1788, a similar experiment, substituting 1728 feet of connected wooden laths for the wire, and confirmed Dr. Hooke's results ‡.

* 'If a rod of iron or brass be held with one end to the ear, and the other be struck upon, it makes a much greater sound than the same stroke upon the rod, when not so contiguous to the ear. By which, and other instances, it should seem that sounds do not only slide upon the surface of a smooth body, but also communicate with the spirits in the pores of the body.'—*Sylva Sylvarum*, Phonics, § 3.

'The pneumatical part, which is in all tangible bodies, and has some affinity with air, performs, after a sort, the office of the air. Thus the sound of an empty barrel is in part created by the air on the outside, and in part by that in the inside; for the sound will be less or greater, as the barrel is more or less empty; though it communicates also with the spirit in the wood, through which it passes from the outside to the inside.'—*Sylva Sylvarum*, Phonics, § 2.

† 'And though some famous authors have affirmed it impossible to hear through the thinnest plate of Moscovy glass, yet I know a way by which 'tis easy to hear one speak through a wall a yard thick. It has not yet been thoroughly examined, how far otacoustics may be improved, nor what other ways there may be of *quickening* our hearing, or *conveying* sounds through *other bodies* than the air; for that that is not the only medium I can assure the reader, that I have, by the help of a *distended wire*, propagated the sound to a very considerable distance in an instant, or with as seemingly quick a motion as that of light; at least, incomparably swifter than that which at the same time was propagated through the air; and this not only in a straight line, or direct, but in one bended in many angles.'—Preface to Hooke's 'Micrographia.'

‡ Acad. Berl. Deutsch, abh. 1788, 87.

Other experiments of a similar nature were subsequently r
by Herhold and Rafn*, Hassenfratz and Gay Luss
&c.; but the first direct observations of the actual veloci
sound through solid conductors were made by Biot, assist
different times by Bouvard and Martin. These experim
were made on the sides of the iron conduit-pipes of P
through the length of 951m. 25; and the mean result of
observations made in different ways gave 3459 metres,
11,090 feet per second, for the velocity of sound in
iron ‡.

Previously to these last-mentioned experiments, Chl
had, in an ingenious manner, inferred the velocity of soun
different solid substances; and his results are fully confir
by calculations from other grounds. His method was four
on Newton's demonstration, that sound travels through a s
of a given length, filled with air, in the same time th
column of air of the same length, contained in a tube ope
both ends, makes a single vibration ||. His own disco
of the longitudinal vibrations of solid bodies, which are exa
analogous to the ordinary vibrations of columns of air, enal
him to apply this proposition to solid bodies, and to estab
the general law, that sound is propagated through any ela
substance in the same time in which this substance makes
longitudinal vibration. In this manner he ascertained
velocities of sound in the following substances, among oth
tin 7,800, silver 9,300, copper 12,500, glass and iron 17,5
and various woods from 11,000 to 18,000 feet in a second.

From the experiments of M. Perolle §, it would app
that the intensity with which sound is communicated thro
solid matters is nearly in proportion to the velocity of its tra
mission.

* Reil's Archiv für die Physiologie, vol. iii., No. iii., p. 178.

† Annales de Chimie, tome liii., p. 64.

‡ Mémoires de la Société d'Arcueil, tome ii., p. 403.

|| A single vibration is here considered as the motion of the vibrating between the two opposite limits of its excursion, and with this signification th pression is adopted by Chladni. Other authors, however, regard this, with Ne and Sauveur, as a semi-vibration, and call an entire vibration the motion of vibrating body from one limit of its excursion until it again arrives at the limit. This difference of meaning attached to the same term has given ri several mistakes.

§ Journal de Physique, tome xlix., p. 382.

§ 2.

In all the experiments above alluded to, the sounds transmitted were either mere noises, such as the blow of a hammer, or, as in Herhold and Rafn's experiment, a single musical sound, produced by striking a silver spoon attached to one end of the conducting wire; and in no case were any means employed for the subsequent augmentation of the transmitted sound. I believe that, previous to the experiments which I commenced in 1820, none had been made on the transmission of the modulated sounds of musical instruments; nor had it been shown that sonorous undulations, propagated through solid linear conductors of considerable length, were capable of exciting, in surfaces with which they were in connexion, a quantity of vibratory motion, sufficient to be powerfully audible when communicated through the air. The first experiments of this kind which I made were publicly exhibited in 1821, and notices of them are to be found in the Literary Gazette, Ackerman's Repository, and other periodicals of that year. On June 30, 1823, a paper of mine was read by M. Arago, at the Academy of Sciences in Paris, in which I mentioned these experiments, and a variety of others relating to the passage of sound through rectilinear and bent conductors *. I propose, in the present instance, to give a more complete detail of these experiments than I have yet published; and at the same time to add what additional facts my subsequent experience has furnished me with on the same subject.

§ 3.

Before proceeding any further, it will be necessary to make a few observations on the augmentation of sound which results from the connexion of a vibrating body with other bodies capable of entering into simultaneous vibration with it. This participation of the vibrations of an original sounding body is called *resonance*, or reciprocation of sound.

Sonorous bodies are audible (the extent of their excursions being supposed equal) in proportion to the quantity of their vibrating surfaces. Thus, a plate of glass or metal is capable

* An abridgment appeared in the Ann. de Chimie, July, 1823, and the entire paper in the Annals of Philosophy, August, 1823.

of producing powerful sounds without accessory means; the sound of vibrating bodies of smaller dimensions, suc insulated strings, or tuning-forks, are scarcely audible moderate distance from the ear; but the sounds of the are capable of considerable augmentation when communi to surfaces, as when they are placed to a table, or tc sounding-board of a musical instrument.

There are several circumstances which influence the i sity of the resonance of a sounding-board. The princij these is the plane in which the vibrations of the sounding are made with respect to the reciprocating surface. Thı vibrations may be so communicated as to be perpendicul normal to the surface, in which case the sound is the greatly augmented; or they may be tangential to, or i same plane with the surface, when the sound is the most f The first of these cases may be illustrated by placing a v ing tuning-fork perpendicularly to the surface of a flat b and the second, by placing it perpendicularly to one of the of the board. In intermediate positions—viz. when the tions are communicated obliquely to the surface—the will be found to have intermediate degrees of intensity.

These facts, which the extensive investigations of S place in full evidence, being understood, the peculiarit the sounding-boards of various musical instruments ad easy explanation.

The sounding-board of the piano-forte is better dis than that of any other stringed instrument, as the planes vibrations of the strings are, on account of the directi which they are struck by the hammers, always perpend to its surface. The difference of intensity when a vibrates in this way, and when it vibrates parallel to th face, is very obvious, and may be easily tried by stril with the finger in these two directions *. There is no otł strument now in use, in which the strings make their vibı perpendicular to the sounding-board.

* It sometimes happens, when the impulse is oblique to the direction i the string presses on the bridge, that its plane of vibration assumes a motion; the periodical changes of intensity thus occasioned, produce similar to that of the beating of imperfect unisons. This phenomenon is g erroneously attributed by tuners to a faulty string.

In the guitar, lute, &c., the strings are also parallel to the sounding-board, but the vibrations must, for the convenience of performance, be made obliquely to it. If the sides of the instrument be of inconsiderable depth, and the back be flat, the difference of intensity between the perpendicular and oblique vibrations will be very sensible. But if the sides be deep, very little difference will be perceived, as the vibrations which are tangential to the front sounding-board are perpendicular to the sides, which thus enter readily into normal vibrations; this fact may be proved by placing the ear to the side of a guitar while a string is made to sound with its plane of vibration successively parallel and perpendicular to it. In some instruments, as the lute, mandoline, &c. the back is polygonal or curved; by this construction a considerable portion of the resonant surface enters into normal or nearly normal vibrations when the strings are struck obliquely to the principal sounding-board.

These laws are not so immediately applicable to the violin, and other instruments of the same class; an extensive series of experiments will yet be necessary to enable us to account for the peculiarities of their forms, their various curvatures, and the functions of that irregular conductor, resting on the sounding-board at two points only, which in these instruments is called the bridge. The investigations of Savart still leave much to be desired on this head.

In no instrument are the strings perpendicular to the sounding-board; for in such case, however a string were made to vibrate, its communicated vibrations would be tangential. But they are sometimes placed obliquely, as in the harp, and then the same changes of intensity may be observed as when the strings are parallel to the board; for if the plane of their vibrations coincide with that of the inclination of the board, the communicated vibrations of the board will be oblique to its surface, and the intensity will be at its maximum; but if they be perpendicular to this plane, the communicated vibrations must be tangential to this surface, and consequently the intensity will be at its minimum.

Besides the proper adaptation of sounding-boards, there are other circumstances on which the tones of a stringed instrument

materially depend; one of the most important of these
proper dimensions of the volume of air contained with
sides; the laws of these resonant cavities have occupi
attention of Savart, but the obvious use of the bars
within these cavities to divide the mass of air, and t
enable it to vibrate more readily in separate portions, se
have escaped his notice.

§ 4.

In the piano-forte, the guitar, &c., the ends of the
are not in immediate contact with the sounding-board, bu
rest on bars of wood, which are called bridges, through
the vibrations are communicated to the board. In the
struments the bridge is usually about half an inch in h
and in the violoncello does not exceed three inches.
certain how far the distance might be extended betwe
string and the sounding-board of a piano-forte without
to the tone, I substituted a glass rod five feet in length
bridge, and by placing at its end a string stretched on
bow, I found that the sound of the string was as dis
audible as when it was immediately in contact with the
a tuning-fork placed at the end of the rod gave the
result. These experiments, which were the first I made
subject, and which suggested all the subsequent ones,
been repeated in the theatre of the Royal Institution
larger scale. A series of connected deal rods, forty
length, was suspended so as to extend, in a straight lin
liquely from an open window of the cupola, to within
distance of the floor of the room; on the upper end
conductor, an assistant placed the stem of a vibrating
fork; when no sounding-board was placed at the lower
mity of the conductor, no sound was heard, but it b
powerfully audible the instant the communication was
this experiment was repeated with different acute and
toned tuning-forks, employed both in combination and i
cession.

Tuning-forks are the most convenient instruments for
experiments on the transmission of sound, because their

tions are almost inaudible by themselves, and only become strongly audible when augmented by resonant surfaces.

§ 5.

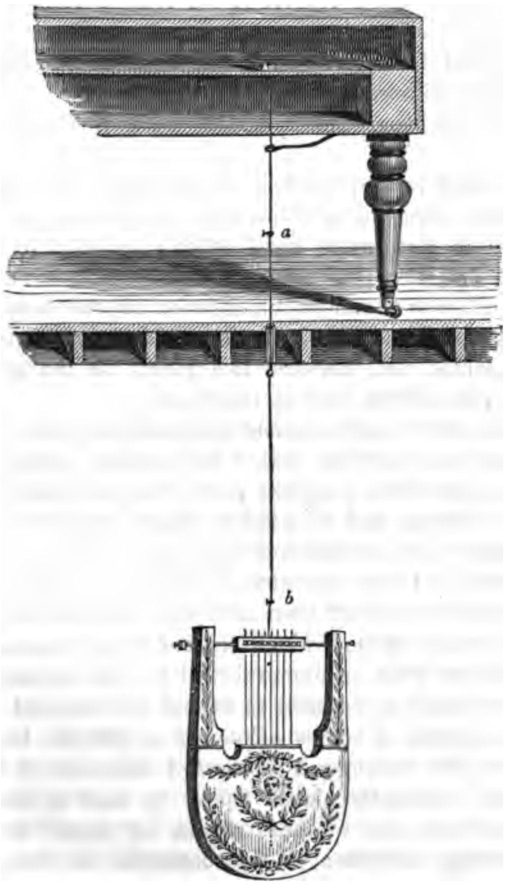
The vibrations of the sounding-board of any stringed instrument may be communicated in the same manner as those of a string, or of a tuning fork, to a distant sounding-board by means of a metallic, glass, or wooden conductor; but in this case it is necessary to prevent the original sounds from being heard through the air, otherwise the communicated sounds will not be distinguishable from them. This may be effected by placing the originally vibrating, and the reciprocating instruments in different rooms, and allowing the conductors to pass through the floor or wall separating the two rooms.

In the passage of the conducting-rod or wire through these partitions, care must be taken to prevent its touching their sides; for this purpose, a tin tube, covered at its two ends with leather, or India rubber, may be inserted in the partition, and the conductor be made to pass through holes in these coverings, so as not to touch the side of the tube.

A square piano-forte is a very convenient instrument to employ in these experiments. If the sound is to be transmitted upwards, nothing more is requisite than to open or remove the lid of the instrument, and to allow the conductor to rest upon the sounding-board. A metallic wire is not sufficiently rigid to support itself thus without bending; a rod of some straight-fibred wood, as lancewood or deal, is therefore better adapted for this form of the experiment; the lower end of the rod must be reduced in thickness, so as to allow it to pass between two adjacent strings; and the best place to make the contact will be found to be about a quarter of an inch from the bridge, among the middle notes, and on the side occupied by the unvibrating portions of the strings. The reciprocating instrument in the room above, may be a guitar or any other similar instrument, or a harp; in which latter case, the rod may be brought in contact with the inner surface of the belly of the instrument, through one of the apertures of the swell. These were the forms under which the experiments have been repeated at the Royal Institution.

If the sounds of the piano-forte are to be transmitted c
wards, a brass wire, about the thickness of a goose-quil
suffice for the communication, as the weight of a reciproc
instrument suspended from it below will keep it suffic
straight. To bring the conducting-wire into contact wit
under surface of the sounding-board of the piano-fort
aperture must be made in the bottom of the instrument i
diately below the intended point of contact; and to ens
perfect connexion with the sounding-board, it is advisal
furnish the wire with a shoulder just below its entranc
the aperture, and to occasion an upward pressure o
shoulder, by a piece of leather stretched on a ring (as
insulating-tube above described) and placed at the end
strong steel spring; the other end of which is screwed
to the bottom of the instrument. To assist in supportir
wire, another shoulder may be made on it, so as to rest
the upper covering of the insulating-tube which passes th
the floor; and the reciprocating instrument may be susp
by inserting the end of the wire into the sounding-boarc
then securing it by a nut and screw on the opposite side.
form of the resounding instrument is a matter of choice
in order to obtain the freest and loudest tones, it is requi
have the principal vibrating surface perpendicular to th
ducting-wire. It is instructive to observe the gradual cl
in the intensity and the quality of the transmitted s
when the sounding-board is made to pass through the v
degrees of obliquity from a perpendicular direction to th
ductor, until it is in the same plane with it; or, to e
Savart's language, as the communicated vibrations c
from normal to tangential; in the latter case, the sound
a subdued, and what is ordinarily called a metallic q
In the first public experiments I made in 1821, the re
cating instrument, which was the representation of an a
lyre, was so constructed as to produce tangential vibratio
tones were consequently far inferior to what I have sinc
able to produce. The transmitted sounds are not s
impaired when the wire is separated at several places, a
disunited parts fastened together by mechanical contac
annexed wood-cut represents the divisions of the condu

wire, which I found it convenient to make in the original form of the experiment, for the sake of portability and facility of removal; but, if the apparatus be intended as a fixture, it will be easier and better to employ but one length of wire. The wire consisted of four portions: the first part touched the sounding-board of the piano-forte, and reached half-way to the floor; the second passed through the insulating-tube in the floor, and terminated when it reached the ceiling of the



room below in a hook; a third part was suspended frc hook by a loop; and the fourth, after identifying itse one of the apparent wires of the lyre, passed within strument, and was ultimately fixed, at its lower end, point marked at the end of the dotted line on the soı board; each of the disunited parts were allowed to each other at *a* and *b*, and were fastened together by of a clamp with a screw-nut. The whole apparatus th pared may be easily removed; the clamps being un: and the resounding instrument removed, the lower wiı be unhooked from the ceiling, the hook unscrewed, : middle wire withdrawn from the insulating-tube: the t fixing or removing the apparatus need not exceed minutes.

From what has preceded, it will be obvious in what two square piano-fortes or two harps may be so conne mutually to reciprocate each other's sounds; by s arrangement, two performers in different rooms may duet together to two distinct audiences, or one may e performance of the other. If the transmission is reqı be horizontal, *i.e.*, between two rooms on the sam cabinet piano-fortes must be employed.

The sounds of an instrument may be at the same tim mitted to more than one place; for instance, commun may be made from a square piano-forte to a resoun strument above, and to another below; and the co cation may be even continued through a series of recip instruments. If the instruments be not in adjacent but be further removed from each other, a person in tl mediate room, through which the conductor passes, v no sound but what is communicated by the ordinary Hence it would be possible to extend a horizontal cc through a series of rooms belonging to different hou: (provided the instrument connected with one of itı mities be constantly played upon) to hear at plea: performance in any of these rooms, by merely atta reciprocating instrument to the conductor; on remov instrument, the sonorous undulations would pass iı

to the next apartment. These observations will equally apply to the transmission of other musical sounds, which will be hereafter noticed (§ 6, 7).

§ 6.

The transmission of the sounds of those stringed instruments which produce sustained sounds, as the violin, violoncello, &c., is equally effective. The conducting-rod may be applied either to the back or the front of the instrument; no precise directions can be given with respect to the points at which the contact should be made; but, in general, the effect has appeared to me better when the end of the conductor has not been too far removed from the situation of the sound-post.

§ 7.

I have been able to effect the transmission of the sounds of reed wind-instruments through solid conductors as perfectly as that of instruments dependent on the vibrations of sounding-boards. In the clarionet, or any other reed instrument, the column of air and the vibrating tongue (or reed) mutually influence each other in such a manner, that whether the sounds be communicated to the atmosphere from the column of air, or to a solid conductor from the vibrating tongue, the quality (timbre) of the sound undergoes no change.

To connect the conducting wire, which may be of brass, and about a tenth of an inch in diameter, with the tongue of the clarionet, the end of the wire must be bent for about a quarter of an inch, and then filed flat on both sides. This flattened end must be fastened to the fixed end of the tongue by the silk wrapping which usually fastens the tongue only, and the angle of the bend be adjusted so as to suit the position of the performer. If the sound is to be transmitted downwards, the embouchure of the clarionet must be placed in the performer's mouth in the usual way, viz. the tongue of the reed resting on the under lip; but if the sound is to be transmitted upwards, the performer must play, as some eminent masters of this instrument do, with the tongue applied to the upper lip. For the bassoon or the hautbois, it is equally convenient to the

R 2

performer, whether the wire be applied to the reed ab below.

The resounding instrument may, as in the expe above detailed, be either a harp, a piano-forte, or a It is a singular effect to hear the sounds of a wind-inst thus reproduced by a sounding-board.

§ 8.

The experiments I have made with respect to other cl wind-instruments have not been equally successful. I possible to communicate the vibrations of the air to conductor without an enormous loss of intensity: if, h the intermediation of other bodies which enter readi vibration, from the agitations of the air, be employ transmission may in some measure be effected. Thus end of the conducting-wire be placed in the most s vibrating part of the column of air in a flute, there is b perceptible transmission of sound; but if the wire to side of the instrument, it will more readily transmit the as the side is susceptible of entering into vibration. E this latter case, the sounds are scarcely audible, unless be held close to the resounding instrument.

In a similar manner, the sounds of an entire or may be transmitted, viz. by connecting the end of th ductor with a properly constructed sounding-board, so as to resound to all the instruments. The effect of periment of this kind is very pleasing; the sounds, have so little intensity as scarcely to be heard at a from the reciprocating instrument; but on placing the e to it, a diminutive band is heard, in which all the ments preserve their distinctive qualities; and the pia fortes, the crescendos and diminuendos, their relati trasts. Compared with an ordinary band, heard at tance through the air, the effect is as a landscape miniature beauty through a concave lens, as compar the same scene viewed by the ordinary vision through a atmosphere.

§ 9.

In the preceding experiments on the transmission of sound through solid bodies, the conductors have been represented as straight; but, though sound is transmitted the more readily through straight conductors, it will yet pass, though with diminished intensity, through rods with angular and curved bendings. If a vibrating tuning-fork be placed at one end of a straight brass rod, the other end of which rests perpendicularly upon a sounding-board, the vibrations will, in accordance with what has been above stated, be powerfully transmitted; on gradually bending the rod at any part of its length, while the vibrations of the tuning-fork are kept in the same plane with the angle of the bent rod, the transmitted sound will progressively decrease in intensity, and will be very feeble when the angle has become a right one: as the bending is continued so as to make the angle between the two parts of the rod more acute, the intensity of the sound will increase in the same order in which it had before diminished; and when the two parts of the rod are nearly parallel, the sound will be nearly as loud as when the transmission was rectilineal. If, during the gradual bending of the rod, the plane of the vibrations of the tuning-fork be perpendicular to the plane of the angle made by the two parts of the rod, the same changes will be observed, but in a more obvious manner, than in the former case; and when the angle becomes a right one, the sound will be scarcely perceptible. At intermediate inclinations of the two planes, the gradations of intensity, occasioned by the bending of the rod, will be found to be intermediate.

The changes of intensity dependent on the variation of the angle of the two planes may be instructively shewn by bending the rod permanently to a right angle, and placing, as before, the stem of a tuning-fork so as to form the prolongation of one of the parts of the rod, the other part of the rod resting on the sounding-board. On gradually turning the tuning-fork round the axis of its stem, without inclining it to the rod, the plane of the vibrations will assume every angle with respect to the plane in which the two parts of the rod is bent. During the revolution it will be observed, that when the planes coincide

the intensity will be at its maximum, and when they a pendicular to each other, at its minimum; thus, suppos sound to commence when the two planes are parallel gradually diminish until they make an angle of 90° then increase through the same changes of intensity inverted order, until it acquires its maximum at 180°; again decrease between this and 270°, and increase arrives at its first position at 0°. If the stem of the fork be placed perpendicularly on the side of a conduct resting on a sounding-board, the same phenomena i observed; the stem of the tuning-fork is, in fact, a sho ductor, forming a right angle with the rod.

Were it necessary for the transmission of sound t undulations should propagate themselves only rectilinea obvious that they would not pass through a bent rod; the other hand, had they the property of diffusing the equally in all directions, we should not observe any diff of intensity in the experiments above noticed. These riments lead us to conclude, that sound diffuses itsel directions, though unequally; that it is communicate readily in the plane in which the original vibrations are and with the greatest degree of intensity in the direc these vibrations.

§ 10.

In most of the experiments relating to the transmis the sounds of musical instruments, which I have in the ing paragraphs detailed, the conductor has been repı as receiving its impulses from a surface vibrating norn which it was perpendicularly attached; the commun was consequently effected by longitudinal undulations conducting wire. But if, while the conductor retains i tion, the surface were to vibrate tangentially or obliqu communication would be effected by transversal or undulations.

In practice it is preferable to employ the longitudina lations for the purpose of transmitting musical soun distance; for, firstly, the transmission is more effic and, secondly, the transverse undulations have a gre

dency to communicate themselves laterally from the conductor to the surrounding medium, and thereby to become audible without the assistance of a reciprocating instrument. This lateral dispersion is scarcely observable with small conductors but is very obvious when a rod of considerable diameter is employed.

I had an opportunity of observing this fact while repeating some of the preceding experiments at the Royal Institution. A square piano-forte was placed in the apartment beneath the lecture-room; and a conductor, placed perpendicularly to its sounding-board, passed through the floor separating the two rooms, but no reciprocating sounding-board was placed at its upper end. By this arrangement, longitudinal undulations were communicated to the conductor; and, whether this was a brass wire one-tenth of an inch in diameter, or a square deal rod half an inch thick, the insulation appeared to be equally perfect. But it was not so when the conductor, instead of being placed on the sounding-board of a piano-forte, was made to rest on the top of the bridge of a violin, and the strings, put into vibration by drawing a bow across them, communicated transverse vibrations to the conductor; it was now observed, that the metal wire insulated the sound tolerably well, but that when the wooden rod was employed, the sound communicated to the air from the entire surface of the portion of the rod above the floor, was nearly as loud as if a sounding-board were placed at its extremity.

§ 11.

I have in this paper given the general results of a variety of experiments made at different and distant periods during the last ten years; but they are far from forming so complete a course as I have been desirous of making. To extend these experiments much farther would be attended with some difficulties: but as the velocity of sound is much greater in solid substances than in air, it is not improbable that the transmission of sound through solid conductors, and its subsequent reciprocation, may hereafter be applied to many useful purposes. Sound travels through the air at the rate of 1142 feet in a second of time; but it is communicated through iron wire,

glass, cane, or deal-wood rods, with the velocity c
18,000 feet per second, so that it would travel the dis
200 miles in less than a minute.

When sound is allowed to diffuse itself in all direc
from a centre, its intensity, according to theory, decr
the square of the distance increases; but if it be con
one rectilinear direction, no diminution of intensity o
take place. But this is on the supposition that the c
ing body possesses perfect homogeneity, and is unifor
structure,—conditions which never obtain in our actual
ments. Could any conducting substance be rendered p
equal in density and elasticity, so as to allow the und
to proceed with a uniform velocity without any reflec
interferences, it would be as easy to transmit sounds
such conductors from Aberdeen to London, as it is
establish a communication from one chamber to a
Whether any substance can be rendered thus homogene
uniform remains for future philosophers to determine.

The transmission to distant places, and the multip
of musical performances, are objects of far less imp
than the conveyance of the articulations of speech.
found by experiment that all these articulations, as wel
musical inflexions of the voice, may be perfectly, though
transmitted to any of the previously described recipi
instruments by connecting the conductor, either imm
with some part of the neck or head contiguous to the
or with a sounding-board, to which the mouth of the
or singer is closely applied. The almost hopeless diffi
communicating sounds produced in air with sufficient i
to solid bodies, might induce us to despair of further s
but could articulations similar to those enounced by the
organs of speech be produced immediately in solid bodie
transmission might be effected with any required de
intensity. Some recent investigations lead us to hope
are not far from effecting these desiderata; and if
articulations were once thus obtained, the constructio
machine for the arrangement of them into syllables,
and sentences, would demand no knowledge beyond t
already possess.

ON THE INFLUENCE OF THE 'SENSE' OF MUSCULAR ACTION IN CONNEXION WITH VISION.

By ALEXANDER SHAW, Esq.

TO those philosophers who have studied the subject of sensation, the organ of vision has ever been the most attractive; and yet the opinions of men, whose attainments we must respect, differ very widely as to the mode of operation of this external organ of sense.

Dr. Brewster, in his 'Treatise on Optics,' which is the latest publication on this subject, has introduced an explanation of the problem—Why an inverted image on the retina should give us the idea of an erect object. This question, as he himself has remarked, has been a frequent cause of perplexity to the learned; but I am of opinion that no theory of vision can be admitted to be correct, which does not afford a satisfactory explanation of it.

This problem involves certain physiological principles, which are not generally understood, but upon which the doctrines of perception through the organs of the senses, especially the eye, have the most intimate dependence. As the solution, which Dr. Brewster has adopted, is unconnected, and, indeed, altogether at variance with these, it becomes necessary to examine it with care; and I think it can be shown, notwithstanding the confident tone in which he has expressed himself, and the high authority which he deservedly holds in questions of optics, that the explanation which he has offered is liable to many powerful objections. It is the more important to undertake this examination, since the view which he has presented may be considered as that which generally prevails. It is the same explanation which was originally proposed by Dr. Porterfield in the 'Medical Essays,' and Dr. Reid, when treating of the mode of operation of the organ of vision in his 'Inquiry into the Human Mind,' has explained it upon the same grounds.

These writers have all agreed in representing that the idea of the 'direction' in which objects are seen is obtained immediately from the retina or nerve of vision. They suppose that this nerve can convey to the mind a sensation of the course in

which the rays have proceeded from the object to
upon it; or, in other words, that the retina can rec
impression, not only of the object, but of the direction i
the object is presented to it. Founding upon this a
position, they find it easy to frame what they consider
just explanation of the problem. In an inverted imag
assert, the retina does not convey the impression of it
cular parts being inverted; but each point in the in
judged to be in the direction in which the rays have pro
in falling upon it; the uppermost pencil of rays from the
falling upon the lowest part of the image, gives the sensa
its proceeding from the highest part, and consequently
that part appear to be at the top, instead of at the b
and so, they say, it holds with regard to the lowest ray
all the others. To use Dr. Brewster's words, the retina
along the lines of visible direction;' that is, the lines
lead from the image in the direction of the object.

Such is the leading proposition on which the whole
is rested. But it is surely an error to assume that the
possesses such a power as is here attributed to it. If we
What is the meaning conveyed by the words 'visible direc
—it will be seen, on reflection, that they include some
more than a simple sensation obtained through an org
sense. To acquire the idea conveyed by the term 'dire
alone, it is necessary that there should be a comparison; t
to say, an operation of the mind itself. We can only for
idea of the particular quarter or situation in which a b
placed, by informing ourselves of its position in rega
another, which has been previously fixed upon as the sta
of our comparison. To say, then, that our knowledge of
rection' can be obtained at once, and can be conveyed t
sensorium like an impression through the optic nerve,
employ the term in a vague and loose manner, which
necessarily lead into error.

It is contrary to all analogy to attribute to a single n
as the optic, the possession of such incongruous powers a
theory assumes. Allowing that the idea of 'direction' c
be conveyed to the mind through the medium of a nerv
would follow, if this theory were correct, that the optic r
was not only sensible of the relative position of an object,

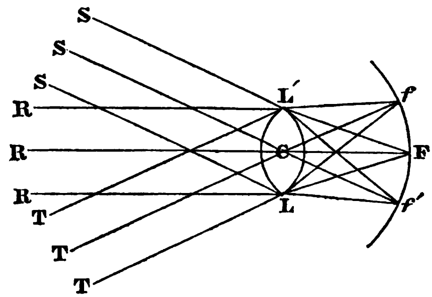
that it had, at the same time, the sensation of the variations of light which distinguish that object from others placed around it. The usual expression of Dr. Brewster, that the image on the retina is 'seen along' a certain line, implies this: for these two words signify that the retina, besides discovering that the object is red, blue, or yellow, determines that it is placed to the right or to the left, or above or below, and also the exact line or degree in which it is so placed.

The main error, which has misled the numerous writers who have treated of this question, and which has created the degree of puzzle that seems always to have been attached to it, may be traced to this—that they have invariably sought to solve the problem by a reference to the functions of the optic nerve alone; they have looked upon the globe of the eye and this nerve as constituting the entire instrument of vision; without taking into consideration the apparatus of muscles, and their nerves, which, under the guidance of the will, move and direct the eyeball from point to point. It can only have been from taking this partial view of the organ of vision that they could attribute to the optic nerve such complicated and inconsistent functions as those which have been bestowed upon it.

The explanation of the problem—Why an inverted image should give the idea of an erect object, which has been adopted by Dr. Brewster, is founded upon the law which has been called the 'law of visible direction.' This supposed law, as the words themselves imply, includes the opinion that seeing and the power of distinguishing the direction of objects are possessed by the retina together; and the following passage, taken from the 'Treatise,' will serve to exhibit the nature of the proofs upon which it rests.

'*On the Law of Visible Direction.*—When a ray of light 'falls upon the retina, and gives us vision of the point of an 'object from which it proceeds, it becomes an interesting ques- 'tion to determine in what direction the object will be seen, 'reckoning from the point where it falls upon the retina. In '*fig.* 142, let F be a point of the retina, on which the image of 'a point of a distant object is formed by means of the crystal- 'line lens, supposed to be at L′ L. Now the rays which formed

'the image of the point at F, fall upon the retina in all
'directions from L′ F to L F; and we know that the p
'seen in the direction F C R. In the same manner th
'*f f′* are seen somewhere in the directions *f′* S, *f* T.
'lines, F R, *f′* S, *f* T, which may be called the *lines* o



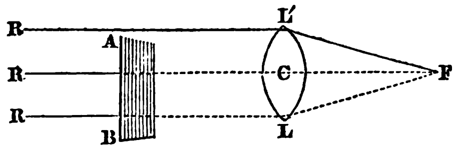
'*direction*, may either be those which pass through the
'C of the lens L′ L, or, in the case of the eye, through the
'of the lens, equivalent to all the refractions employed
'ducing the image; or it may be the resultant of all the
'tions within the angles L′ F L, L′ *f* L; or it may be
'perpendicular to the retina at F *f′ f*. In order to det
'this point, let us look over the top of the card at the p
'the object whose image is at F, till the edge of the card
'about to hide it; or, what is the same thing, let us obstr
'the rays that pass through the pupil excepting the upp
'R L′; we shall then find that the point whose image is
'is seen in the same direction as it was seen by all the ray
'C F, L F. If we look beneath the card in a similar m
'so as to see the object by the lowermost ray R L F, w
'see it in the same direction.'

It is remarkable that Dr. Brewster, in conducting an ment of this nature, should have fallen into two such err are here exhibited.

1. He endeavours to demonstrate how the retina ca cover the relative direction of an object, and yet he altogether to present any second object or image to our n In each of the three instances which he has given, h confined his attention solely to the object whose direction i the question at issue, or to the rays which come from it,

the image formed by it upon the retina. But it is obvious that from such a mode of proceeding no conclusion could ever be drawn. There ought to have been some standard, or some fixed point introduced, by which to calculate the comparative direction of the object; or to enable us to estimate the truth of the demonstration, by contrasting one line of visible direction with another.

2. But it is in his experiment that the most remarkable error is found: indeed, the conclusion which he has drawn from it must have somewhat startled the reader. He affirms that if the rays RC RL, proceeding from a distant point, be cut off by a card (which may be represented by AB), and the



ray RL′, alone enters within the eye, the image F will be seen along the line FCR. But how can this, by any possibility, be the case? Is it not obvious that the card AB totally obstructs vision in that direction? if we 'see along' the line, it can only be by seeing through the card! An explanation seems to be required how Dr. Brewster could have been brought to make so extraordinary a statement. The error can only have been occasioned by his proceeding on a wrong path in making this inquiry. Unexpected conclusions are often forced upon us in the study of mathematics; and even when a strict course of inductive reasoning is pursued in physical subjects, the results are often strange, and apparently paradoxical: so that we need not be surprised that a learned philosopher should occasionally yield his assent to a proposition which an uneducated person, guided solely by his common sense, would at once reject. But it does not appear that the conclusion which Dr. Brewster has drawn, in the present instance, is to be justified on the grounds of its being founded either on mathematical demonstration, or on correct induction. In explaining the result of his experiment, he has mixed up with the facts a bare and unwarranted assumption regarding the power which belongs to the retina.

If the experiment with the card be made as he has it will be found to be true that the image F retains position, whichever ray, proceeding from the object mitted to enter the eye; and it appears in the san whether all the rays, or only one be admitted. But tl means leads to the conclusion that the direction of t is ascertained by the retina pursuing the object along ticular line. It shows only, that the same sensation is whether a ray falls obliquely upon the retina, either fro or from below, or falls upon it in a perpendicular. li that, therefore, no contrast can be made between th ought rather to have been noticed that all the rays external object concentrate towards a single point in tl F—that each individual ray, however separated from th in its course, must proceed from the same point in the and affect the same spot in the retina—that, conse no difference in the sensation is to be expected—and is altogether futile to look upon the direction of the rays as leading to any knowledge of the place of the ob

It is not easy to understand what is meant by the ex 'seeing along' lines, when these lines stretch outward the retina through the humours of the eye and thro atmosphere. Dr. Porterfield has said that 'by virtu connate and immutable law, the mind traces back its ov sation, and sees every point of the object, not in the ser or retina, but without the eye, in those perpendicular which Dr. Brewster has called 'the lines of visible dir This is supposing us to possess a power which reacl beyond where the optic nerve is present to exercise it. also presuming that the mind has a consciousness of tl ward object, distinct from that sensation which is conve the image formed upon the nerve.

But it may be asked, how is the consciousness of the being external obtained?—how do we acquire the kno of the simple fact, that the body, whose image is painted interior of our eye, is exterior to us? It is not throug medium of the optic nerve alone that this information tained. Our sole knowledge of the existence of an ex body, so far as the nopticerve is concerned, is acquired th

the rays which emanate from it. These rays, being reflected and dispersed to a distance, according to a law of nature, from all the surfaces of the body, pass into the eye, and form upon the retina an exquisitely minute image or copy of the object. Now it is this image alone which gives rise to the impression of the object in the mind. The outward body itself does not directly excite the nerve: we have to rely for the correctness of our knowledge respecting it, upon the image, formed by the rays, being a faithful representation of it. As the image, therefore, is at a distance from its original, and as it is perfectly distinct from it, how do we learn to associate it with the external object?—how do we discover that it is not an ocular spectrum, or a mere phantasm, that we see? for it is known that an impression may be made upon the retina, and remain there even while the object is removed altogether from our presence. We can only be assured that the image represents an object which is not placed within the eye, but is external to our body, by calling in the assistance of, at least, one other sense: more of these being brought in as evidences may strengthen our conviction; but one in addition is absolutely necessary. We have the means of ascertaining the fact which we desire, in the muscular apparatus that always accompanies the possession of an organ of vision.

The muscles have the power of turning the eyeball either towards the object or in a contrary direction. Of this we are conscious. Now, it appears to be a simple conclusion to arrive at—that the object must have a separate existence of its own, and distinct from the eye which perceives it,—when, in order to see the same object, we invariably find that it is necessary to exercise the muscles in a particular manner. We know that, if the body presented to our sight be in motion, as a bird flying through the air, we must follow it with our eyes, making fresh efforts to keep them in the direction of its flight, otherwise it will disappear. If the image were a mere spectrum, as that produced by looking at the sun, it would present itself in whichever direction we happened to turn our eyes. Hence it follows that, even without calling the sense of touch or any of the remaining senses into operation, but depending upon the knowledge acquired from merely shifting the eye about, we

become convinced that the body whose image is in exists externally. To 'trace' or 'see along' a line expression is used, includes the opinion that the placed externally; it likewise includes the idea of gu directing the eye; and, in addition to these, it implies istence of a coloured image painted upon the retin notion conveyed by these words is, therefore, most cated; and they ought never to have been applied, as l done, in speaking of the functions of a single nerve.

Another inconsistency in the theory may be notice theory rests upon the supposition that the retina can guish the direction of the object by seeing along line lead from the image to the external body. But it is prehensible how the nerve, which is seated at the b the eye, should be able to ascertain, by itself, the dire rays or lines which terminate in it. These rays of li only sensible when they arrive at that point in the su the retina which is their final destination; and they recognizable by any other nerve of sense besides th nerve. Now, according to the most elementary definit mathematics, when we desire to learn the direction of it is necessary that two points in it, at least, should b known; but, according to the theory, it is the singl alone which terminates the 'line of visible direction' made sensible. If we could conceive that the line, in through the anterior surface of the eye, caused a sensa the particular spot in the cornea where it entered, we then have two points presented to the mind, by which mate the direction of its passage from the object to the but, without such a second point, it appears quite imp to ascertain its direction.

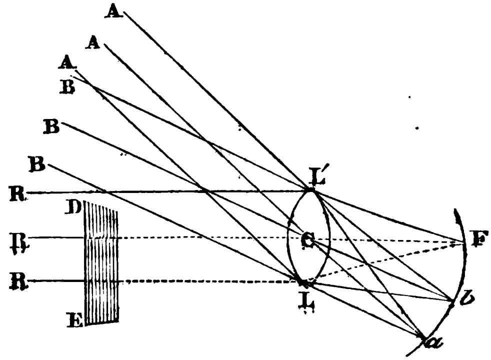
The question as to the manner in which the idea 'direction' of objects is obtained, is to be approac a very different way than by attending to the rays proceed from them: it requires a more complex operat the senses to acquire this knowledge of surrounding than has been conceived. We have already seen that, in to ascertain the simple truth that an object is situated nally, something more is necessary than a mere impr

made upon the optic nerve; and the same will be the case, it is natural to conclude, if we seek to discover its exact place in the external world. When we desire to find out the 'direction' of a body, our true purpose is to ascertain what is its position in relation to other bodies or to some fixed standard. It is sufficiently obvious that the object itself, if it were altogether detached from surrounding bodies, could never present to the mind this idea. To entertain the conception implied by the term 'direction,' it is required that other objects be presented to the sight besides that which is under our particular view; and to institute the proper comparison, or to form the necessary calculation, it is as indispensable that the sensations of these neighbouring objects should be communicated to the mind as that the sensation of the object itself should be so presented. However varied the direction of the rays proceeding from a single object may be when compared with one another, no reasoning upon them can ever acquaint us with the relative position of that body to another, whose rays are not given. Dr. Brewster has forgotten to supply any second body, or to represent its rays, although to establish a 'line of visible direction' the existence of some standard of comparisou is necessarily implied.

When an object is presented before the eye, the surface of the retina is not occupied with it alone, but the whole surrounding scene, everything which is near it and can be included within the field of vision, is represented upon the nerve at the same time. Now, these are so many impressions by which the mind can judge of the relative position of objects; and so long as these find admission into the eye, *data* shall not be wanting for instituting the necessary comparison. If a diagram be drawn, representing the surrounding objects and their images falling upon the retina, it will be easy to understand how the mind acquires the knowledge of the direction of each of them. We may make the diagram correspond with the experiment of Dr. Brewster, as it will serve as well as any other to demonstrate what is required.

The card D E being placed before the eye, may prevent objects situated in that direction from casting their images upon the retina; but, according to the terms of the experi-

ment, it does not interfere with the ray R L′. This admitted, is refracted to the same point, F, in the which all the other rays, if they had not been shut c



also have concentrated. The ray R L′ being allowed the eye, there is no reason why objects placed above should not also have their images represented upon th Let *b* and *a* be such images, formed by the rays B It is obvious that these preserve the same relation to th at F as they did before the card was used. Now, t explanation of our seeing this, or, in other words, of ou taining any thing respecting the position of F, is this is, first, an impression of the image F conveyed thro optic nerve to the sensorium; then the image *b* is pı to it; and, in succession, the image *a* comes before it,– cess of comparison, which the mind alone can perform stituted between these various images, and the result knowledge of the relative direction of F to *b* and *a*.

It is thus that the surrounding bodies, as well as the cular object of sight, require to be included within the of observation, in order to obtain the idea of 'direction to the intermediate process by which these objects are s sively presented to the mind, this is a question whic braces subjects of the highest interest.

I would remark, in the first place, that the eye is not a and motionless instrument, and the retina is not posses an uniform degree of sensibility throughout all its surface of which things the theory which I have been consi would seem to imply. The retina has one spot in its su

situated in the axis of vision, which is more acutely sensible to the rays of light than any other part; and the eyeball is admirably provided to turn and present its transparent surface so that it may catch the impressions of objects upon this spot. It is only when images fall upon this part of the retina that the mind is satisfied, or that a perfect sensation is obtained; and the power of guiding the eye with this intention, whether it be through the motions of the body or of the head, or is produced directly by the action of the muscles of the eye, is as necessary to the perception of the direction of the things around us, as the groping about with the hands is to a blind person, to enable him to find his way through the confused furniture of a room. We are sensible of this searching motion of the eye, made in preparation for distinct vision; and it is by calculating the extent of this motion that we estimate 'direction.'

In common language we are accustomed to speak of 'directing' the eye: it is allowed also, that it is by a voluntary act that we accomplish this; but it is not so generally conceded that the exercise of the muscles, by which the eye is moved, communicates a distinct sensation to the mind; and yet it forms an important part in the process of vision. When an astronomer directs his telescope to the heavens, he is aware that his instrument combines in it two separate means of acquiring the knowledge which he seeks. By the apparatus of lenses or mirrors inclosed within the tube, he ascertains the magnitude, brilliancy, or colour of the star; but it is by a perfectly distinct apparatus that he determines its place in the heavens. He consults, for this purpose, the external parts of his instrument, such as the various levers, screws, and joints by which the tube is revolved; and it is by looking to the scale attached to his telescope, that he is informed of the exact bearing or of the direction of the particular star. Now the outward apparatus of muscles by which the eyeball is directed in vision, is surely, in an inquiry of this kind, deserving of attention. There is a sensibility resident in the muscles which gives token of the degree of their contractions; and it is by attending to these impressions, like looking to the scale of the telescope, that we become conscious of the direction of objects.

This sensibility to the exercise of the muscular independent of sight, or any of the other organs of tl it is altogether a distinct source of sensation to If a person were blind, or completely isolated from ward objects of sense, he would still be sensible of th of his arms, or of his body, or of his eyes; and if a st comparison could only be communicated to him, he able to tell in which direction and to what extent l them. Although it has been only lately recognize 'muscular sense' exists in the body, yet it appear distinctly one as any of the five senses, smelling hearing, taste, or touch. Anatomy discloses to us th whose office it is to convey sensations merely, and v incapable of influencing the muscles to contract, are di with profusion to all the voluntary muscles throug body. These sensitive nerves of the muscles, it has b cluded by Sir Charles Bell, convey to the brain the cc ness of the contraction of the muscles which have b viously excited through the proper nerves of motion establish a communication, as it were, in a circle, betw muscles and the sensorium, whose office it is to regu extent of the action of the muscles; and they carry mind that knowledge of the condition of the muscles, which their actions could neither be controlled nor a nor be under the guidance of volition.

Let us observe what takes place during vision, and perceive how intimately this 'sense' of the muscular is connected with the question before us. When we an object placed high above us, the first thing which w rally do is to throw back the head, to turn the face the skies, to elevate the upper eyelids, and to raise the of both eyes upwards. Now these actions are not per without our knowledge. If we have to inspect an object is placed on one side, we may be obliged to wheel rou fore we can see it; at all events it will be necessary the eyeball in the socket towards that side. If we h examine an object placed at our feet, there is first a sponding motion of the eyeball and of the head, or it r of the whole body, in order to be enabled to look down

These motions of the frame, accompanying vision, are familiar to all persons. We can even tell, at a distance, in what direction a person is looking, by observing the position of his body; and if we can see his eyes, we may tell whether he is looking at ourselves, or the particular spot that engages his attention. The boxer or the fencer knows full well how much the motion of the eye has to do with seeing; for it is by watching keenly the eye of his adversary, that he learns the exact place where the blow is to be struck, and can parry it. There is invariably associated, therefore, with seeing, a particular position of the organ of vision; and if it be allowed that we possess the consciousness of this position of our organ, it must, I think, be concluded that this is the source of our ideas of direction. We contrast the position of the eye necessary for seeing one object with distinctness, with that which is required for seeing another. Certain standards of comparison are arbitrarily fixed upon, and it is by referring to these that we assert that an object is placed high or low, to one side or to the other. If we turn our eyes upwards from the ground, we say that the object is high; if we direct it downwards, we say that it is low: and, in the same manner, we say that it is placed to the right or to the left side, according to the direction in which the eyeball is revolved when looking upon it.

Thus it would appear that the motions of the body and of the eyeball together, constitute an important part in our perception through the organ of vision. The consciousness of the action of the muscles accompanies the sensation which the retina bestows; and it is the almost simultaneous reception of these two different kinds of sensation, added to the effects of early habit in associating them, that gives rise to the common feeling of their both being obtained from the exercise of the same sense.

If we now apply this view of the manner in which the 'direction' of objects is discovered, to the problem of 'erect vision from an inverted image,' it will afford an easy explanation of it. I ought first to state, that there are no reasonable grounds for the notion, that an inverted image upon the retina must necessarily be attended with an impression of the object, which is looked at, being also inverted. The opinion has been held,

that originally, as during infancy, all objects are seen inverte and that some process is required to correct this false impre sion. But there appears to be no reason for entertaining su a conception. The connexion established between the ima upon the retina and the mind which receives the sensation, altogether so incomprehensible, that no distinction can be su posed to depend upon the image being either inverted or ere There would be an absurdity in speaking of the image bei inverted in reference to the mind, which is incorporeal, as speak of it being inverted in reference to the eye or the exter object; and the process of sensation would not be a whit m intelligible, if the image were placed erect instead of being verted. It is more just to believe that the image, of itself, give no impression whatever of the position of the object, only those sensations which proceed from light, as the varie of colour, brightness, shadow, outline, &c. The question is sim How does the idea of direction first enter the mind?—how we ascertain that the base of an object is placed towards ground, and its top towards the skies? And this question r be considered as one altogether independent of the positio the image at the bottom of the eye.

If we proceed upon the principle which has been st above, the answer to this question must be—that we judg the direction of the various parts of a body by ascertainin what position we must place the eye, in order to see it tinctly. When a tree, for example, is presented to our we direct the eyes downwards to observe its trunk roote the earth—we turn them upwards to see its uppermost bra and we turn them to each side to see the right and left of the tree: and it is by referring to these motions tha conclude that one part is above, or another is below. In these motions, however rapidly performed, a distinct sens accompanies the change; and this is communicated to the as surely as is the impression upon the retina itself. If we s for an analogy, we might find it in the hand; for the l exactly the same by which we take an object, and touch upon one extremity, we say that is its top; and touching it another, we say that is its bottom. We observe what i extent of motion of the arm in reaching from its highest e

mity to its lowest; and it is by this sensation, combined with the sensation communicated to the skin, that we determine the position of the object through the sense of touch.

The most remarkable circumstance connected with this subject is the minuteness and the precision with which the eye can observe the differences of place or direction in objects. This can only be explained by referring to the extraordinary fineness of the sensibility to the different degrees of light which belongs to the spot of the retina situated in the axis of vision, and to the susceptibility of the muscles to perceive the smallest variations in the position of this spot while engaged in directing it towards objects. Things which, from their minuteness, almost elude our naked sight, can be divided into upper, lower, and lateral parts.

The conclusions to be derived from this mode of explaining the nature of a perception through the organ of vision, are both curious and highly interesting. We learn that the ideas of objects, which we are in the habit of saying are acquired through the eye, are never the productions of that one organ; but, as if it were for the purpose of certifying the reality of the things around us, and placing this reality beyond the doubts of philosophers, who may have been bold enough to question it, two distinct senses are called into operation. Thus before we obtain the assurance of the simple fact, that the tree before us has its trunk fixed in the ground, and its leaves in the air, the optic nerve must, in the first place, convey the representation of the tree, that is, its colours, shadows, and outlines, by which it is distinguished from other objects; while, in the second place, the muscles must cause the eye to traverse all its boundaries, taking points, and marking distances, so as to estimate its height and breadth, figure and position. There are thus not only two senses, but a process of comparison, calculation, and judgment, which implies an operation of the mental powers, combined in making this simple perception of a tree complete.

Without going further into this subject, I may be allowed to remark, that to the medical man it is of much importance to study the mode of operation of the organs of the senses, and particularly of that which has been under our consideration. The questions connected with squinting and disordered actions of the muscles of the eye, which are still so little understood,

may derive assistance from attending to the intimate connexi which has been shown to exist between the retina and the muscl When it is observed that the state of activity of the optic ner draws along with it an activity of the muscles, and that bo these parts are equally engaged in the simplest act of perce tion, it is to be expected that the derangement of the one w materially affect the other. But the nature of these actions the muscles of the eye cannot be properly understood, unless attend also to the involuntary motions of the eyeball; by which t eye, at the instant that the optic nerve falls into a state of repo becomes subject to the operation of a distinct class of muscl The consideration of these subjects, together with the study the complicated process by which an act of perception is co pleted, may perhaps throw some light upon the questions disordered vision, hallucinations, and some of the delusions the mind arising from false perceptions of the objects of sig For the principles on which these views of the nature of visi are founded, I beg to refer my reader to the 'Essay on Sing Vision,' by Dr. Wells; where the first indications will found of the knowledge of a 'muscular sense;' to the posth mous writings of Dr. Brown; but more particularly to t papers published by Sir Charles Bell, on the nerves whic supply muscles, and on the nerves and muscles of the orbit.

ON THE INDURATION OF CHALK AND CHALK EART UNDER WATER, AND THE APPLICATION OF THIS PR PERTY IN HYDRAULIC ARCHITECTURE.

By J. PENNISTON.

[In a letter to Dr. Fowler, of Salisbury.]

Dear Sir,

IN compliance with your wish, I will relate to you, in writi the practical observations I have been enabled to make the properties which chalky substances have of consolidati and hardening in the water.

The first circumstance of any consequence that occurr to me in this way was previous to the repair of Harnha Bridge, which it was my professional duty to direct as Survey for the county of Wilts.

I was then induced, on the recommendation of the forem

of the masons, to make the bays of chalk, or rather of chalky earth, which forms the banks adjoining the turnpike-road at the bottom of Harnham Hill.

The stream, as you are aware, at the upper side of this bridge, runs extremely rapid, and I confess I had my doubts whether this material would consolidate in so narrow a width (not exceeding 2½ feet), and confined only between two hurdles (such as are commonly used for penning sheep), sufficiently to resist the force of the river.

In the progress of forming this bay, a considerable portion of the finer particles of the earth washed away with the current, but sufficient remained to answer every purpose intended. It was formed by men treading in the earth; and on the evening of the day on which it was made, the whole substance was like a bog, or quagmire, where pressure on any part operated on the whole bulk; but on the following morning it was a perfect wall: it continued for many weeks impervious to the pressure of the stream, and when it was necessary to remove it, it presented so obstinate a resistance, that several pickaxes were broken in attempting to do so; nor was it until the following summer, when the water was lower, that it was fully cleared away, and then with the same labour and loss of iron as before.

The next proof I had of its utility, was at Burford Bridge (a village just above Amesbury, Wilts). Here the pier of a cast-iron bridge was literally underwashed, and in great part destroyed, by the floods of 1823-4, and the bed of the river so ploughed up, as to be in holes of from five to ten feet deep. These, after restoring the piers, and repairing the bridge, I filled up by driving piles, and ramming in between these piles *large* rubble chalk, clay, and flints; but in spite of the care I took in the execution of that plan, the floods of the succeeding winter cleared it completely out. A great portion of the lumps of chalk were rolled some scores of yards down the stream. The piles and clay vanished altogether.

The specimens I had had of the chalk earth, induced me to fill in these holes with the same kind of materials; and I employed some horses and carts to bring it from a chalk-pit at some little distance, selecting such as had been pulverized by wet and frost, and carefully discarding the larger lumps.

The immediate effect of this operation was more surprisi than the former: as the carts were backed into the river, a their contents tipped into the stream, the consolidation w almost immediate, for as the carts successively came with th loads, the parts which had been previously filled in were c pable of bearing the *wheels*, with their loads on them. T whole wash was filled in at a comparatively slight expens and remains perfect to this hour.

From these proofs it has occurred to me, that if the sar material were used to strengthen the bed of the Thames in t line of the Tunnel yet to be excavated, it might be attend with the happiest results. All that would be required, wou be to bring in barges a sufficient quantity of chalk earth, a throw it into the river at low water: the current would do t remainder of the work; nor have I a doubt, if it prov necessary to cut through a portion of this new-made bed some future excavation, that it might be done with as mu security as cutting through a solid rock of chalk.

Having the honour of being acquainted with Mr. Timot Bramah, I have mentioned to him the result of the exper ments I have here described, and my opinion of their applic bility, if the Tunnel should be renewed. The trial would n be a very expensive one, and I should be too happy if ar suggestion of mine could, in the slightest degree, be benefici in forwarding a work so nationally desirable to be completed.

I am, dear Sir, &c. &c.

July 16, 1831. J. Penniston.

FURTHER OBSERVATIONS UPON SILICEOUS DEPOSIT FROM THE URINE.

By ROBERT VENABLES, M. B.,

Physician to the Chelmsford Provident Society, &c. &c.

THE existence of silex in the urine has been generall admitted, upon the authority of Berzelius, and he believe that it is accidentally derived from the water which we drin Silex has been found, in three instances, intermixed with th composition of urinary calculi; in two by Fourcroy an

Vauquelin, and in one by Professor Wurzer. I believe I was the first, so far as I have been able to ascertain, publicly to notice the deposition of crystallized silex as gravel from the urine, the particulars of which have been already detailed in an earlier number of this Journal. At the period of sending the communication, and for some considerable time afterwards, I continued to enjoy frequent opportunities of witnessing the appearance of this deposit. It was not, however, a constant occurrence, but was occasionally interrupted for a time, and at various intervals reappeared. Upon one occasion, I transmitted the gravel, precisely as passed, to a gentleman * much devoted to researches of this description, with a request that he would acquaint me with his views upon the subject; and upon receiving his reply, I was much chagrined to learn that he had not been able to discover any siliceous matter in the specimen with which I had furnished him. As I placed the utmost reliance upon the correctness of this gentleman's conceptions, and the accuracy of his judgment, I almost regretted that I had committed my paper to the press, the more especially as, upon several after occasions, the gravel exhibited all the *sensible* characters of the siliceous depositions previously observed; but upon *chemical* examination, it did not afford even a trace of silex. However, it was not long before I was gratified by the reappearance of the silex, a portion of which —the specimens being passed at different times—I sent to my friend, and found that he confirmed me in the fact of their siliceous nature. The only question with him *then* was their urinary origin; but upon this subject I fully satisfied myself by having the urine passed in my presence, so as to prevent the possibility of practising any deception.

Since the publication of that paper, Dr. Yelloly, of Norwich, discovered silex in small granules, 'imbedded in the substance of an oxalate of lime calculus,' from the Norwich collection. The calculus weighed about five grains, and was taken from a boy about nine years old. The examination of this calculus, with the chemical proofs of the intermixture of siliceous granules, is detailed at length in Dr. Yelloly's paper, published in the Philosophical Transactions. Dr. Yelloly, in consequence

* Dr. Prout.

of my paper in this Journal, favoured me with a con
tion upon this interesting subject, and, at his request,
honour of furnishing him with some specimens of sile
by my patient. In comparing them with those disco
himself, he observes that 'they bear some resemblance
they are more minute and are of an amber tinge)
which I have mentioned as coming under my own viev

Very lately I had another opportunity of detecti
small siliceous particles in a crystallized form in some
fragments passed by a patient of Mr. Green's, who ha
to be visiting at this place. Having accidentally met w
gentleman, he was mentioning the circumstances of h
particularly his frequently voiding quantities of gravelly
Having obtained a specimen recently passed, I found t
consist of lithic acid with volatile and fixed alkali and lim
volatile alkali was evolved by heating with caustic potassa, a
came sensible by the pungent smell. The presence of fixe
was proved by fusing a particle with a small quantity o
finely divided silex. Exposed on charcoal to the flame
blow-pipe, the mass fused into a globule, the transpare
which was different in different instances. The gravel wa
pale cream colour, and seemed like so many fragments or
of the outer covering of a small nucleus, having both a
cave and convex surface. Among these I found a
nucleus of an irregularly rounded shape, and of about the
of a snipe shot. It was insoluble in muriatic acid, but so
with effervescence in the nitric.

Among the fragments which were of a cream-colour, I
served several which were of a much whiter appearance,
about the bulk of mustard-seed shot, irregular in shape. F
their appearance I took them to be the mixed or *fusible* p
phates, but upon urging a particle with the blow-pipe, I
surprised to find that it underwent little or no observa
change, except that during the ignition it assumed a very br
or brilliant incandescence. After ignition it had a stron
alkaline reaction, and when moistened with distilled wate
slaked and fell to powder like caustic lime, which an additio
quantity of the water dissolved. On subjecting this solut

* On the Tendency to Calculus Disorders. Phil. Trans. 1830.

to the action of a stream of carbonic acid gas, from a capillary jet, it became turbid, and a white powder subsided, soluble with effervescence in diluted hydrochloric acid, and which was again precipitated by oxalate of ammonia. Hence there can be no doubt that the base of this particle was lime.

Another fragment of the same external characters being placed on a capsule, was dissolved with considerable effervescence by a few drops of diluted hydrochloric acid. The whole, however, was not entirely dissolved, but there remained at the bottom of the capsule three very minute crystals, which the acid could not dissolve, though aided by heat. The muriatic solution was very carefully withdrawn, and on being neutralized and heated with oxalate of ammonia, oxalate of lime precipitated. The three crystals were now carefully washed and removed to a platinum capsule, and boiled with concentrated nitric acid, but without undergoing the slightest perceptible change. The acid was driven off by evaporation, and the crystals submitted to the action of the blow-pipe on the platinum capsule, but without suffering any alteration. A little potassa and soda being added, on urging with the blow-pipe flame, solution with effervescence was effected, and the whole fused into a perfectly transparent colourless globule. The globule being pulverized and heated with distilled water, hydrochloric acid being added in excess, a gelatinous mass of very small bulk subsided after a considerable interval. When the jelly had consolidated into a closer and much less bulky deposit, which it did after a couple of days, the supernatant fluid was carefully withdrawn, and the precipitate being well washed with hot muriatic acid, was transferred to a small capsule, and the whole evaporated to dryness, leaving behind a white insoluble powder, which resisted the most intense action of the blow-pipe.

This analysis, therefore, fully proves that the little fragment, instead of being, as I at first imagined, composed of the fusible phosphates, consisted of carbonate of lime, with a very minute though still sensible proportion of crystallized silex. Hence then it would appear that silex, though rarely, does occasionally appear in a crystallized form in the urine. Berzelius, indeed, estimates its quantity at .03 in the thousand parts, but

upon this subject I may observe that it is not always pres in the urine of the human subject, nor is its appearance const even when occasionally discoverable. I have repeatedly analy the urine of the same individual, and have at times rea found distinct and satisfactory traces of silex; while at oth after the most careful and minute investigation,* I have b unable to discover the slightest indication of the presenc this substance.

Being anxious to ascertain whether the silex was confi to this one fragment, or whether it existed in any propor in the remainder, a quantity, amounting to a grain in weig was taken promiscuously from the mass, and being introdu into a test tube, and tolerably strong nitric acid added, was applied. It dissolved with effervescence. Distilled w was now added, and the tube, fixed in its stand, was pl under a glass jar and left at rest for forty-eight hours. T was no deposition whatever, which would have been the if there had been any intermixture of silex. Ammonia b added till neutralization was *nearly* effected, oxalate of monia precipitated a considerable proportion of oxala lime. The presence of lithic acid was proved by expos portion to the action of caustic potassa in excess aided by and then pouring off the clear solution. Acetic acid added in excess, the precipitate was washed and collecte a capsule. The solution of this precipitate in nitric acid evaporated to dryness, and then acted on by ammonia, p the presence of lithic acid by the formation of *purpur* ammonia.

The presence of carbonic acid in the white limy-lo particles was proved as follows: A very small test tube filled with and inverted over mercury, a particle or two calcareous carbonate was introduced, and immediately r the top of the mercury. A small quantity of moderat luted hydrochloric acid was introduced, by means of a test tube, into that containing the carbonate. The dilute immediately rose to the surface of the mercury in the and, acting on the carbonate, dissolved it with consid effervescence; the mercury at the same time descend

* The entire specimen with which I was furnished did not amount to two

the tube. Nitrogen gas was now introduced from a capillary pipe connected with a bladder of this gas into the tube, till the whole of the mercury and the muriatic solution were expelled. A longer tube being filled with and inverted over mercury, about two drachms of lime-water was passed into it, and rose to the top of the quicksilver. The gas of the first tube was now passed into the second: the lime-water became muddy, and a diminution in volume succeeded from the absorption of the carbonic acid, the nitrogen remaining behind*. The recently formed carbonate being acted on by diluted hydrochloric acid, dissolved again with effervescence. The muriatic solution being collected by a pipette, was precipitated by oxalate of ammonia in the usual way,—thus proving the composition to be carbonic acid united to lime.

The little nucleus which, it has been noticed, was placed in a capsule and subjected to the action of nitric acid, next attracted my attention. The capsule had been left at rest for several days under a glass, and on examining it there was found a very minute residue of an indistinctly crystalline appearance, but of high refractive density. The capsule was heated and the acid boiled, but the boiling effected no solution. After subsidence, the acid was removed as carefully as possible; it was then evaporated till the whole of the acid was driven off. The insoluble residue was now ignited by the flame of the lamp enlivened by the blow-pipe directed upon the mass in the capsule: it effectually resisted the heat, undergoing no alteration whatever; but, on adding a little soda, it melted with effervescence into a transparent convex button, flattened at the bottom by the shape of the capsule, thus proving the siliceous character of the residue.

Hence, then, it appears that, in this specimen of gravel, there were distinct traces of silex in two separate fragments. In the one, the silex was in a crystallized form and intermixed with carbonate of lime: in the other, it was in an amorphous or pulverulent form, and mixed with lithic acid combined with an alkaline and earthy base†. If I had

* This was the most easy and ready process with so minute a quantity.

† From the nitric solution carbonate of potass precipitated carbonate of lime in large proportion, soluble with effervescence in hydrochloric acid, and reprecipitable by oxalate of ammonia.

detected the silex in the carbonate of lime fragment ‹
the minute portion, and no silex being discoverab
general mass, I might have been tempted to sup
the carbonate of lime was an accidental impurity,
mately referable to a urinary source, although it is n
credit or even suspect that the whole of the intermi
ments of carbonate of lime were mere accidental
nations.

But the detection of silex imbedded in lithic salts l
doubt of its urinary connexion; and this circumstanc
the urinary origin of the crystallized specimen no lon
vocal. What the peculiar circumstances are which d
the mode of appearance, it is difficult, nay impossibl
present state of our knowledge, to determine. The n
earthy diathesis seems to prevail, the greater the ten
the silex to separate in the crystallized form; and, in
far as the few facts ascertained upon this subject will
an inference, the deposition of silex seems connected
earthy diathesis. Fourcroy and Vauquelin found the
phosphate of lime; Dr. Yelloly found it in oxalate o
Professor Wurzer found it in a calculus consisting pri
of lithic acid with a small proportion of phosphate c
Wurzer's calculus yielded 1 per cent. on analysis. It
position was as follows:—

Phosphate of lime	17.33
Lithic acid	75.34
Animal matter	6.33
Silex	1.00
	100.00

Alemani gives the chemical composition of a urina
culus, containing not only silica in large proportion, bu
phosphate of iron. It was as under:—

Magnesia	51.00
Silica	20.00
Phosphate of iron	21.84
Carbonate of magnesia	4.00
Loss	3.16
	100.00

However, the composition of this concretion is so e

ordinary that, allowing the analysis—of which, however, the details are not given—to be correct, doubts may be fairly entertained of its urinary origin. From its composition, it is more legitimately referable to the class of mineral productions.

In the case noticed, in an earlier number of this Journal, as occurring to me, and in another, with the circumstances of which I was not personally so well acquainted, the urine showed a great tendency to alkalescence. They were both women; and one of them, Newton, died lately, after having been much afflicted; the other has left this neighbourhood, and I have not heard of her for some considerable time. I regret much that I had not any opportunity of examining the urine in the present case, the gentleman having quitted this neighbourhood the day following that on which I saw him. I understood him to say that he passed water with great difficulty and pain, and that much exertion brought on a discharge of blood. I understood also that, on sounding, no calculus could be discovered; and, indeed, the shape and size of the fragments tend to prove that, if formed, it must be of very small size. I have generally observed that the secretion of much earthy matter is connected with an alkaline diathesis, and, indeed, soon induces disease of the bladder.

The circumstances which give rise to the appearance of silex in the urine are enveloped in the utmost obscurity. I believe it has not been observed to separate from the urine (after being passed) in a crystallized form spontaneously, nor can it be effected by art. I certainly once observed a deposition of something like crystallized silex on the sides of a tall jar, in which the urine of one of the patients, whose case I have described at length in an earlier number of this Journal, had been suffered to stand for several days; but I must observe upon this subject that, owing to an accident, I had not an opportunity of verifying my supposition by a chemical examination. The quantity observed in the present case is so minute, that possibly it may be looked upon rather as an accidental ingredient. To this, however, it may be objected, that its appearance in the crystallized form is not exactly compatible with such a view. That pulverulent, or finely comminuted silex, might be introduced with drink into the stomach, and pass (in the

gelatinous state), as fluids frequently do, to the kidney less circuitous route than the circulation, it is possibl ceive; but that it could pass in a crystallized state, this shorter route, or through the more circuitous o circulation, I think will not readily be admitted. Th other way, then, of accounting for the appearance of gular deposit, but its secretion by the kidney, and it tion, as other morbid concretions. Nor is there more in conceiving the kidney capable, under certain circun of such an elimination, than an operation of which t be no question—the formation of the CYSTIC OXIDE.

NOTES UPON VEGETABLE TISSUE. By JOHN LI Esq., F.R.S., &c.

No. 1.—*Cellular Tissue.*

BOTANISTS generally recognize three principal ele forms of tissue, of which, under a variety of modifi all the parts of plants are constituted; these forms are lular, the fibrous, and the vascular. As far as regards ti a state of perfect organization, the limits of these divisi sufficiently exact, and the latter may be understood as e tary forms; but, if we consider tissue with reference to constituent parts, we shall find that these three principa are constructed of something still more elementary: viz., brane and fibre; and that, under the head of cellular tiss really comprehended certain modifications, composed thing but the latter elementary matter.

Cellular tissue is well known to be the basis of vege to be that form which is indispensable to the existence vegetable being, and, therefore, to be in all cases pr while the two other forms are present or absent in plan cording to their species. In its most common state, it cc of numerous minute, imperforate, transparent vesicles, pr the one against the other, and by this pressure acquiring va figures, such as the dodecaedral, the prismatic, the colu the cubical, &c.; its sides are destitute of all markings, e

such as may arise from the adhesion of grains of grumous matter to them, and evidently consist of nothing but a very delicate membrane. This, which is the general character of cellular tissue, is not unfrequently considered its absolute distinction; it appears, however, from recent observations, that it is subject to some very remarkable modifications.

It is an old idea, that the membrane of all tissue is composed of interlaced fibres, but this opinion seems to have originated in theoretical views, and either not to have been founded upon observation at all, or at least, not upon accurate observation; the existence of such fibres, in any case, has been denied by Mirbel, Link, and others; and it must be evident to any one, that, in cellular tissue, generally, no trace of them is visible. Moldenhauer, however, noticed, so long since as 1779, that the cellules of the leaf of Sphagnum obtusifolium are marked by fibres twisted spirally; but this met with scarcely any attention. Link states, that his supposed fibres are nothing but the lines where small cells, contained in a larger one, unite together; and other botanists pass by the subject without remark. It is, nevertheless, certain, that the observation of Moldenhauer was perfectly correct, and that the cellular tissue of Sphagnum consists of a membrane, within which a fibre is twisted in an irregularly spiral manner; it also appears that this kind of structure is far from uncommon. In November, 1827, I described the tissue of the testa of Maurandya Barclaiana (*See Botanical Register*, t. 1108) as consisting of cellules, formed of spiral threads crossing each other, interlaced from the base to the apex, and connected by a membrane; this was named, at the time, *reticulated cellular tissue*, and an approach to it has since been remarked in the seed-coat of several Bignoniaceæ. In 1828, Dr. Mohl stated, that in the pith of Rubus odoratus, he had seen cellules, the walls of which were marked with delicate fibres having a reticulated appearance; and that other cases existed, in which the fibres (instead of being reticulated) formed curved lines, parallel with each other (see his Memoir *über die poren des Pflanzen-zellgewebes*). In August of the same year, I was so fortunate as to discover upon the testa of Collomia linearis, the existence of incredible numbers of spiral fibres, 'lying coiled up spire within spire, and confined by a dry

mucus, so as to be unable to manifest themselves; but instant water is applied, the mucus dissolves, and ceases counteract the elasticity of the spiral vessels, (spires,) wh then dart forward at right angles with the testa, each carry with it a sheath of mucus, in which it for a long time rem enveloped, as if in a membranous case.' (*See Botanical gister*, t. 1166.) I, however, fell into the error of conside them spiral vessels; they are no doubt analogous to th forms of cellular tissue, in which a fibre only is develo and are probably of the same nature as what Mr. Brown scribed, in 1814, as spiral vessels in the testa of Casuarina.

These cases had clearly demonstrated the coexistenc both membrane and fibre, in the cellular tissue, and also th the latter is usually found composed of membrane only, with fibre, it is occasionally composed of fibre without membra

Besides these instances, Meyen, about this time, descr fibrous cellules in anthers. It was not, however, till last y that the existence of fibrous cellular tissue was proved to b extremely common in flowering plants, that scarcely a species be named in which it does not exist abundantly. It appears the descriptions of Dr. I. E. Purkinje, (*de cellulis anthera fibrosis*, &c.) who, however, does not appear to have been a of the abovementioned observations, that in anthers, the i lining of the valves consists exclusively, either of membra cellules, the sides of which are marked by fibres, arranged e spirally or otherwise, or of fibres only, arising from the cu projecting into the cavity of the anther, and unconnecte any membrane. This statement is illustrated by good fig of nearly three hundred instances, the accuracy of son which I have so verified, that I feel confidence in that o remainder.

It seems probable that this structure, now that attentio been called to it, will be found far from uncommon i cellular tissue of other parts of plants. I have observed the leaves of Brassavola tuberculata in the same state (a imperfect one) as Dr. Mohl found it, and as I have n seen it in Rubus odoratus: and it exists in a state of bea perfection in the leaves of Oncidium altissimum, where of the cellules, much larger than the rest, are evic

formed by the development of a spiral fibre within a membrane.

This being the case, there seems to be no doubt that the basis of cellular tissue is both membrane and fibre, and that the character hitherto assigned to it will require to be much modified in consequence. It may possibly become more difficult to define the exact difference between cellular and vascular tissue: but it will be much less difficult to understand the origin of the latter; and a knowledge of the real character of the former will explain the presence of such tissue as the Elateres of Jungermannia among cellular plants, without the necessity of supposing the existence in them of partial tendency to vascularity.

(*To be continued.*)

ON HARRIOT'S PAPERS. By S. P. RIGAUD, M.A. F.R.S.

Savil. Prof. of Astronomy.

[To the Editors of the Journal of the Royal Institution.]

Gentlemen, *Oxford, Sept.* 1, 1831.

ALLOW me to request that you would correct a mistatement which appears in the third Number of your Journal. Neither you, nor the author of it, could have been aware of the injustice of the accusation which it conveys, or I am confident that I should not have occasion to make this appeal to you.

In a very curious dissertation on the first invention of telescopes, Dr. Mohl takes occasion to say (page 495 of your first volume), 'it is to be lamented that Harriot's papers and manuscripts are at present buried in one of the libraries of the University of Oxford.' Now the truth is, that these papers and manuscripts are not, and, what is more, never were in any of our libraries. The story is old; but as it appears that an erroneous impression respecting it still exists, it may yet be right to lay the real state of the case before the public.

Harriot lived under the immediate patronage of Henry Earl of Northumberland, from whose daughter the present Earl of Egremont is descended, and from whom he inherited these

papers. Zach, when a young man, was in England w
Count de Bruhl, who married the dowager Lady Egremc
and by this means he got access to the manuscripts at P
worth. He found Harriot's papers there in 1784, and e
in 1786 he made a proposal to the University of Oxford
prepare a portion of them for the press, if they would unc
take the expense of the publication. This was immedia
acceded to, and in April 1786, he wrote a long Latin lette
what he intended that his first volume should contain. A
of Harriot, written in imitation of Gassendi's lives of Pu
chius, Regiomontanus, Copernicus, and Tycho Brahe,
to make up the first part, and it was to be followed by s
original observations of the comets of 1607 and 1618. U
the receipt of this communication, an order was made, at
next meeting of the delegates of the press, for the printin
be proceeded in as soon as the editor was ready. Noth
however, was done by him; and after a lapse of eight years
sent, in May 1794, by Bishop Cleaver, then Principal of Bra
nose, not the work which he had promised, completed and r
for the press, but a certain number of the original manuscr
without any of the apparatus which he was to have draw
for them. In the intermediate time he had printed an
count of the papers in the Astronomical Ephemeris of
Royal Society of Berlin for 1788, which was translated
English, and circulated in this country. It was prob
drawn up from loose memoranda: it is easy to unders
that the pleasure of his discovery might have led him to
rate what he had found; but such a feeling will not acc
for the very erroneous statement of facts which he gave,
which may be attributed to an imperfect recollection o
particulars which he intended to describe. He likewise pri
in Supplement I. to Bode's Jahrbuch (1793), an accou
the observations of the comets of 1607 and 1618. Thi
probably what he had intended for a part of his first vol
and if so, it not only marks the time when he had abanc
his original intention, but gives us such a specimen o
work as diminishes the regret which might be felt for hi
having gone on with it.

To return, however, to 1794. The delegates of the

had every wish to promote the publication; but things were now materially altered. They had undertaken to enable Zach to bring out a work, which he professed to be preparing for publication; but he had not only gone back from his engagement, but had thrown them into a situation, in which he would have made them responsible for working up the materials which he had thought proper to select for them. This last circumstance corroborates a correction, which must be made with respect to the manner in which the business was managed. The general idea is simply that Zach, having found these papers, the Earl of Egremont, in consequence, sent them to Oxford for publication; in that case, however, he would most probably have sent the whole, that a judgment might be formed of the connexion and value of the several parts; but the truth is that Zach, from the beginning, merely endeavoured to make the most of his discovery for himself. He applied to the university, in the first instance, to print his work, because, as he expressed himself in his Latin letter of proposals, 'quo tempore et quo auxilio in lucem proferretur, nulla erat post tot tantosve conatus spes relicta, nullum relictum consilium.' When he had given up this object he printed the observations of the comets (possibly the only part which really called for publication), and then made his retreat, so as to turn the eyes of the world from himself to the university. Nothing, of course, could have been done with the papers without his having previously obtained the permission of the nobleman to whom they belonged; but Zach appears to have made himself the prime agent in the whole business, so that no direct intercourse took place between the university and the Earl of Egremont, till after the undertaking had been finally given up. I suspect that the delegates themselves were not apprised of their only having a portion of the papers; at least I can recollect no allusion to it from those of former days with whom I have conversed on the subject, and the fact has certainly not been generally known; but, however this may have been, it had become necessary for those who acted on the part of the university, to take the precaution of inquiring further, and ascertaining the nature and character of what had been put into their hands. The late Dr. Robertson was, therefore, re-

quested to make a report on those papers which were c
with abstract mathematics; and the astronomical part
for the same purpose, into the hands of the late Mr.
of Balliol. This took place in July 1794. I men
specific dates to show that I am not writing from va
ditionary accounts, but from precise documents which
in existence. In the following October, Dr. Robertson
that what had been submitted to him was not calcul
publication; and here the matter rested for some tim
Powell had been prevented from attending to the busin
the delegates, therefore, at length referred his part of the
also to Dr. Robertson. His opinion, which he gave w
reasons for it in 1798, was in this case likewise agai
publication; and in the following year the whole was r
to the Earl of Egremont.

In spite of the fear of being tedious, I have thought
to enter into all these particulars, because they prove
blame, in the slightest degree, attaches to the university.
indeed, could have been brought forward if these fac
been generally understood. But unfortunately they we
Dr. Hutton inserted Zach's account of the papers in hi
tionary (Art. Harriot), and, without sufficient inquiry,
'It is with pleasure I can announce that they (Ha
papers) are in a fair train to be published; they have
presented to the University of Oxford, on condition of
printing them; with a view to which they have been late
into the hands of an ingenious member of that learned
to arrange and prepare them for the press.' The first e
of the Dictionary was printed in 1796, and some allo
might then be made for misapprehension; but Dr. Hutto
the old personal friend of Dr. Robertson, and might hav
tained any information that he desired from him on the
ject; some correction ought, therefore, to have been introd
in the second edition of 1815. Zach's erroneous stater
however, was reprinted in that and many other books
here and abroad: in some cases the substance of Dr. Hut
inaccurate addition was annexed, and the story was repe
till the whole of it was received as authentic. In this ma
obloquy has been brought upon Oxford, because Zach

too eagerly raised expectations which did not admit of being fulfilled.

By the kindness of the Earl of Egremont I have recently been entrusted with these very papers; some of them are very curious, but, as far as I have had time to examine their contents, I have found very little which it would be useful to publish. I can, however, refer you in this respect to better authority than my own. In 1822, Dr. Robertson communicated his reports on them to Dr. Brewster, and they were printed in the Edinburgh Philosophical Journal (vol. vi. p. 314). They refer, of course, only to those papers which were sent to Oxford; of the remainder any one in London may form his own opinion, as a considerable quantity of them was given, I believe, in 1810, by Lord Egremont, to the British Museum. There were, before that time, some other MSS. of Harriot's in the same place (No. 6001-2, and 6083 of the Harleian collection); Antony Wood (Ath. Ox. vol. i. p. 391, 1st. ed.) speaks of a MS. of his, entitled 'Ephemiris Chyrometrica,' which he says was in the library of Sion College, but no mention of it is to be found in Reading's Catalogue; and, although Harriot was an Oxford man, I am not at present aware of our having any papers of his in any of our libraries.

S. P. Rigaud.

ON THE MAGNETIC INFLUENCE EXHIBITED DURING AN AURORA BOREALIS.

By S. H. CHRISTIE, Esq. M.A. F.R.S. &c.

In an account which I gave of some observations on the aurora borealis of the 7th of January last, I stated that it was my intention to adjust a magnetic needle for the purpose of observing the effects produced on it during an aurora, should there be any recurrence of the phenomenon. This I immediately did, but was not fortunate enough to be able to make any observations until the aurora of the 19th of April.

In order that the nature of the action on the needle may be clearly understood, it is necessary that I should point out the

manner in which the needle was adjusted, and how racter of the forces acting upon it may be inferred changes observed in its direction.

I suspended a light needle, six inches in length, brass wire $\frac{1}{100}$th of an inch in diameter, and twe inches long, within a compass-box, having a ring g to thirds of a degree. In the direction of the ax needle, and at equal distances from its centre, we two twelve-inch-bar magnets, the south-pole of ea towards the south. These were made gradually to the needle until their repulsive force on its poles w what greater than the terrestrial directive force. V was the case, there were three positions of the needle the forces acting upon it were in equilibrio, viz., when pole or marked end pointed south—when it pointe where between north and west—and likewise when i somewhere between north and east. At the time observations which I am about to give were made th tions were nearly south, N. 37°W., N. 37° E., and the tions were made on the deviations of the needle pointed between north and west.

Being engaged on the 19th of April in prepari journey, I should not have observed the needle on th ing, nor have been aware of the occurrence of the aur not Mr. Faraday called to inform me of it. A few before ten o'clock, when I first saw it, there was a stream of yellowish light in the west, 12° or 14° in near the horizon, and clearly perceptible to the height although the moon was quite free from cloud: to th there were streamers shooting upwards, and masses light sometimes forming irregular arches. Shortly af there arose from the horizon a very strong stream nearly in the magnetic meridian, 3° or 4° in bread distinct to the height of 50°, and gradually lost tow zenith. This stream continued steady for about four when it gradually disappeared. Immediately after th served the needle, adjusted as I have described.

At 10 h. P. M., I found the needle vibrating N. 43° 40′ W. and N. 42° 40′ W. The vibrations, co

about 1° in extent, gradually increased towards the west, and decreased towards the north, until the needle reached N. 55° 30′ W., at which time there was a strong stream of light from the magnetic north. When this had disappeared, the needle returned gradually, and very steadily, towards north; at length, reaching N. 34°.W.: at 10 h. 15 m., the direction of the needle was N. 34° 40′ W. Soon after this, I again examined the needle, and then made the following observations:

At 10 h.	30 m.	the needle vibrated between N. 40° W. and	N. 39° W.
10	33	the direction of the needle was . .	N. 36 30′ W.
10	35	. . . . . .	N. 35° W.
10	37	. . . .	N. 34 W.
10	37½	. . . . .	N. 33° 30′ W.
10	39	the needle vibrated between N. 34° 20′ W. and	N. 33° 40′ W.
10	42	. . . . N. 36° W. and	N. 35° W.
10	44	. . . . N. 37 W. and	N. 36 W.

At this time there were no streamers, and the light was very faint in the north: barometer 29.94, thermometer 42°. I regret that circumstances would not allow of my continuing my observations throughout the night, which I was very desirous of doing. The next morning at 7 h. 20 m. the needle pointed N. 40° W.

The mean direction of the needle, when uninfluenced by the aurora, I consider to have been N. 37° W. As the needle assumed this position in consequence of the attractive force of the earth, acting on its south pole towards north, and of the repulsive force of the magnet, acting upon the same pole in the opposite direction, a deviation towards west would indicate a diminution in the terrestrial horizontal intensity, and a deviation towards north an increase in that intensity, the intensity of the magnets remaining the same.

In a paper published in the Cambridge Philosophical Transactions for 1820, I first pointed out that the change in the direction of the horizontal needle, arising from extraneous action, would be best determined by referring the action to a needle freely suspended by its centre of gravity, and then referring the direction of this to the horizontal plane; and stated, that in this manner we should be able to account for the changes which have taken place in the variation and dip of the

needle during a long series of years. Taking this vie
subject, Capt. Foster *, by a series of observations
Port Bowen, in 1825, and published in the Phil
Transactions for 1826, showed that the changes in
zontal intensity of the needle might be referred to ch
the dip, the terrestrial intensity in the direction of
remaining constant, or nearly so. If then we cons
during the time of the aurora, the absolute terrestrial
remained constant, the change which I observed in t
tion of the needle indicating a diminution in the h
intensity, it will follow that, during this aurora, t
developed was such as to cause an increase in the di
needle.

The change that took place in the direction of the
was so considerable (more than 21° in less than 15 n
that it required no nicety of observation to mark its
I have before mentioned, that I was so fortunate as
Mr. Faraday with me at the time: the changes were
fest, that he could observe them at a short distance
instrument, at the same time that I was noting the
minutely with the assistance of a glass; and he agreed
that the effects could not be more decisive of the i
exerted upon the needle during the aurora.

It has been stated, that auroræ have occurred, durin
no effect has been observed on the needle; that this
markably the case in Capt. Foster's observations
Bowen; and that these observations are 'a refutation
supposed connexion between tremors of the needle and
borealis.'

With regard to these observations we may remark,
first place, that the needle was observed to be continua
state of tremor, so that it must have been difficult to
whether any effects were produced on the needle dur
time of an aurora; and, in the second, that, although m
effects may, in all cases, be simultaneous with the aur
the direction of the horizontal needle may not invari

* By the untimely death of this meritorious and estimable officer, sc
lost an able, zealous, and indefatigable auxiliary—his friends one whom t
long continue to deplore.

affected. In order, however, to determine how far any effects were manifest in the direction of the needle, we will analyse the abstract of the observations made by Capt. Foster, at Port Bowen, in the months of January and February, 1825, during which months it happens that, in each, the aurora was visible and invisible during the same number of nights as nearly as possible.

Aurora visible. Date. 1825.	Amount of Variation.	*Aurora invisible.* Date. 1825.	Amount of Variation.
January 12	0° 51′	January 1	1° 20½′
15	4 13	2	0 53
16	2 25½	3	0 50
17	2 29	4	0 56½
18	2 56	5	2 33
20	1 08	6	2 50
21	1 17	7	2 03
22	1 20	8, 9	no observations recorded.
24	1 03½	10	1 23
26	2 00	11	2 01½
27	1 55	13	1 00½
28	0 44	14	1 22
29	1 05	19	1 56
30	1 31½	23	1 16
		25	1 12½
		31	0 26
Means	1 47	Means	1 28
February 6	1 27	February 1	0 39
11	3 53	2	0 52½
12	2 46	3	0 17½
13	2 25	4	0 54
14	5 00	5	1 14½
15	4 25	7	0 46½
16	1 41	8	1 10½
19	1 55	9	0 51½
20	1 41	10	0 47
21	1 53½	17	2 46
22	2 10½	18	0 48
23	1 46½	26	1 24½
24	0 19½	27	0 44
25	0 45	28	0 19½
Means	2 17.5	Means	0 58.2
Means, Jan. and Feb.	2 02.7	Means, Jan. and Feb.	1 13.7

From the means, it appears that the variation, c
days on which the aurora was visible, was greater th
those on which it was invisible; in the month of
more than double, and on a mean of the two months
Taking individual observations, we have, in the
January, when the aurora was visible, six days on
variation exceeded the mean variation during the m
eight days on which it was less; when the aurora
visible, five days on which it exceeded, and ten days
it was less than the mean: and in the month of Feb
have, when the aurora was visible, eleven days on v
variation exceeded the mean variation of the month,
three on which it was less; when it was not visible
day on which it exceeded, and thirteen on which it
than the mean. So that, whatever may be the cau
aurora, it is evident from these observations, that, d
occurrence at Port Bowen, the needle had in gener
dency to make wider excursions, although this tende
in many instances, have been counteracted.

I am aware that results, directly the reverse of th
been drawn from Captain Foster's observations. In th
burgh Journal of Science,' vol. viii. p. 200, it is remar
'In the two months during which twenty-eight au
curred, the mean monthly excursions of the needle
side of its mean position was only 1° 37¾'; whereas du
two months when there were no auroræ, it was almost
double, *viz.*, 3° 18′ 41″. If this difference, which is
great to be accidental, shall be confirmed by future
tion, it will prove that, in the arctic latitudes, and i
periods which abound with auroræ, the excursions of th
netic needle are diminished; while, in our latitudes, the
which produce auroræ increase the excursions of the m
needle.'

If we admit this, these observations prove that, du
aurora, magnetic forces are developed, a supposition whi
have been considered to refute; but I think we canno
that the difference here noticed, in the extent of the va
can be fairly connected with the aurora. The mean va
for the month of March, during which the aurora wa

visible on three days, was 2° 14·25′; that for April 2° 52·44, and for May 3° 44·39′, during which latter months the aurora was not seen on any occasion. So that there appears clearly to have been a progressive increase of the variation, quite independent of the aurora; and to this progressive increase we ought to attribute the difference above noticed, more especially as, during the months in which the aurora was visible, the effect appears to have been to increase, instead of diminishing, the variation during an aurora. No one can set a higher value than I do on Captain Foster's observations, but I consider that conclusions have been drawn from them which they do not warrant.

I have already stated, that although magnetic effects may in all cases be simultaneous with the aurora, yet the direction of the horizontal needle may not invariably be affected. This will be evident, if we consider the effects that may be produced on a magnetic needle, freely suspended by its centre of gravity, and refer the direction of this needle to the horizontal plane. If the forces developed are wholly in the vertical plane passing through this needle, it is evident that, although the inclination of the needle may be increased or diminished, yet no change will take place in its horizontal direction, and consequently no changes, in such case, would be observable in the horizontal needle. That this may frequently be the case is evident from the circumstance that the most brilliant beams of the aurora generally affect the magnetic north. I am not aware of the observations which may have been made on the direction of the horizontal needle, during the aurora of the 19th of April last, but as I have before stated, the greatest deviation of the needle which I observed, took place at the time when a strong stream of light issued from the magnetic north. Now, although the effect was here so sensible, owing to the peculiar adjustment of the needle, yet this effect may not have been observable on a horizontal needle, under the influence of terrestrial magnetism alone, however delicately that needle may have been suspended.

If observations were made on a dipping needle, the effects of the forces developed in the plane of the meridian during an

aurora might become sensible; but as this is at l
imperfect instrument, and as these forces would prc
cases be small, compared with the other forces givi
to the needle, the effect produced would, very pı
quite insensible. To obviate this, the directive fc
needle should be diminished, by placing magnets ir
tion of the axis of the dipping needle, with their pol
to the corresponding poles of the needle, the mag
placed at such distances, that the force acting upon
in the direction of the dip should be extremely
observing, however, with a needle so adjusted, it
necessary to be extremely cautious that the instru
the magnets should be so securely fixed, that the
positions could not alter during the observations,—
minute change would produce a very sensible deviati
needle. In order to observe the effects produced by
acting in the meridian, it would be necessary also to
horizontal needle in a similar manner.

As the mechanical difficulties, occurring in such aı
ment of the dipping needle, are considerably greateı
that of a horizontal needle, I consider that it would
to adjust two horizontal needles in this manner :—

1. A light needle being suspended by untwisted
silk, a fine hair, or a very fine wire, two bar magnets
placed as I have described, and at such a distance
marked end or south pole of the needle may still point
Keeping the magnets in the meridian, and still at e
nearly equal distances from the needle, they are to be
approach it until the marked end deviates about 30
east, or west of north. If the needle be now led towar
or south, by means of a small piece of iron held on the
of the compass box, it will remain at 180, provided the
the magnets are in the meridian: if it does not point
the nearer ends of the magnets must be slightly moved
west, without changing their distances, until it does.
magnets should now be firmly fixed in their positions by
nails, and it would be advisable to cover them up with
bad conductor of heat, as a change in their temperatu

cause a change in their intensity, and should this be much diminished, the needle will quit its position at 180°, and resume that at zero. This needle being left in this position, its deviations will indicate corresponding changes in the direction of the magnetic meridian.

2. Another needle should be adjusted in a similar manner, but the magnets should be brought so near to it that the positions in which the needle will rest become 180, N. 70° E. and N. 70° W.; and instead of being left with the marked end pointing to 180°, like the former, it should be led to the position between N. and E., or N. and W. (whichever happens to be most conveniently circumstanced for observation), at which it will remain. The changes in the direction of this needle will principally indicate changes in the terrestrial horizontal intensity, corresponding to changes in the dip.

To those who are desirous of observing the magnetical effects produced during an aurora, and who have leisure to watch for the occurrence of this phenomenon, I would recommend such adjustments of two horizontal needles. During the time of an aurora, these needles should be carefully watched, the observer being very careful to remove from his person every article of steel or iron. Their directions should be noted at very short intervals; if their motion be vibratory, the limits should be marked; and the precise time when any change in the direction of their motion takes place should be carefully noted. Another observer should at the same time note any remarkable circumstance in the aurora,—as the appearance of columns or arches of light, their magnetic bearings and attitudes nearly, with the precise time of their occurrence. It is desirable, also, that the directions of the needles should be observed every day at intervals of an hour, throughout the twenty-four hours, particularly that of the needle pointing south, as this would be but little influenced by changes in the temperature of the magnets. By means of the latter observations, not only the times of maximum east and west variation, and the relative extent of the variation each day would be determined, but a comparison would be afforded between the ordinary diurnal excursions of the needle, and those during an aurora; and they would besides enable us to determine with

greater certainty, whether decided changes in the di[
the needle were simultaneous with the occurrence c
auroræ.

Royal Military Academy, 27*th September*, 1831.

ON THE PHYSICAL CAUSE OF ENDOSMOS

By M. DUTROCHET.

Read to the Académie des Sciences, 25th July, 1831*.

WHEN two liquids, differing in capillary ascen separated by a thin and permeable partition, rents, flowing in opposite directions through this part produced; the *strong current* is that of the liquid whi rise highest, directing itself towards that which woul least, and the *weak current* is that of the liquid whi rise the least, directing itself towards that which w the most. The progressive augmentation of the v the liquid, which would rise the least, is the result of th phenomenon. This augmentation is in proportion to ference which exists between the force of the two oppo rents: it results from the excess of the strong cu compared with the weak current. This excess manif by a simple dynamic effect, for the two opposite cur in equilibrium, or in a state of compensation to the their equal parts. The force resulting from this exce of the endosmosis.

When I first discovered this phenomenon, I was lec sider it as the result of an electric impulsion; and the curious electric phenomenon discovered by M appears susceptible of being referred to endosmosis, n be said not in any manner to differ from it. In thi menon two portions of pure water are separated by brane and electrified, the one positively, and the oth tively, by the two poles of a voltaic pile. The water el

* The Committee have succeeded, by means of their foreign corres establishing arrangements by which they may obtain original comn from abroad for publication in this Journal. The present paper by M. on the important subject of endosmosis, is a paper of this kind.

positively, passes through the membrane to the water electrified negatively, gradually increasing the volume of the latter.

The exact resemblance of this effect to that of endosmosis produced by the difference in the density of the liquids, led me to consider the latter phenomenon as the result of an electric impulsion. The electricity appeared to me to be produced by the difference of density in the two liquids separated by the membrane. Further reflection has, however, induced me to abandon this idea: the body of water in contact with the positive pole disengages oxygen in a state of elasticity; this water, therefore, becomes charged with hydrogen in a state of solution: the body of water in contact with the negative pole disengages hydrogen in a state of elasticity; this water, therefore, becomes charged with oxygen in a state of solution. Thus we have, on one side, water charged with oxygen, and on the other, water charged with hydrogen, or, in other words, two liquids of unequal density. From that moment the phenomenon of endosmosis presents itself, and the water charged with oxygen being necessarily of greater density than that charged with hydrogen, has its volume increased at the expense of the latter. Electricity here is not the immediate, but the remote cause of the phenomenon: it is simply the cause of the difference in density of the two portions of water. This difference is undoubtedly very small, and, therefore, the phenomenon of endosmosis is manifested in a very slight degree.

A very celebrated mathematician (M. Poisson) has sought to explain the phenomena of endosmosis on the principles of capillarity. The following is a summary of the theory which he has recently broached on the subject. The two heterogeneous liquids being introduced into the same capillary canal by its two extremities, are at first both concave, but as soon as they unite, the one remains concave, while the other becomes convex, adapting itself to the concavity of its antagonist; then, by a mechanism founded on calculations made by the learned mathematician, the liquid which rises the highest, and which has remained concave, passes through the membrane, repelling the liquid opposed to it, and runs out: it thus augments the mass of this opposed liquid, with which it is mingled.

Other physiologists, observing that the phenome dosmosis does not take place when two liquids not susceptible of being mixed (such as oil and placed in relation to each other, have supposed that rocal dissolution of the liquids played a principal p phenomenon, and that the gradual augmentation of t of the liquid of the greatest density, was the result of facility of permeation possessed by the liquid of l which was opposed to it. This theory is destroyed l facts; thus, for example, sulphuric acid and water, w the greatest tendency to a mutual dissolution, do nc any endosmosis.

The only mode of arriving at a certain theory upoı nomenon in question, is to observe and appreciate mathematically; this is what I have endeavoured determining, in the first place, the laws regulating and velocity of endosmosis. I have ascertained force is in proportion to the difference in the dens two liquids. Thus, for instance, if a solution of n soda, the density of which is 1·06, be brought int with water, the density of which is 1, there will be endosmosis which will vary according to the extent c of the membrane of the endosmometer. If, in instrument, there be put a solution of the same density of which is 1·12, this solution, being broı relation with water, will produce a force of endosmos will be the double of that produced, under the s cumstances, by the solution having the density The two forces of endosmosis produced by th saline solutions will, therefore, be to each other as excesses of density of these solutions above the densi water, that is : : 00·6 : 0·12, or as 1 : 2. I have end to ascertain whether there was any relation between of the endosmosis and that regulating capillary ascens for that purpose have examined the comparative f capillary ascension of pure water, and of the two sali tions in question. I took a glass tube, the capillary a of which raised water to a height of 12 lines (one in temperature of 10° C. = 50° F., and found that th

tube, under the same circumstances, raised the solution of muriate of soda, the density of which was 1·06, to a height of $9\frac{1}{8}$ lines, and the solution, the density of which was 1·12, to a height of $6\frac{1}{4}$ lines. Hence result the following calculations :—

1st. The capillary ascension of the water being	12.
And that of the first solution..................	9.125
The excess of the capillary ascension of the water is	2.875
2nd. The capillary ascension of the water being	12.
And that of the second solution	6.25
The excess of the capillary ascension of the water is	5.75

These two excesses are precisely in the proportion of 1 to 2, the same proportion which was found resulting from the experiments on the force of endosmosis produced by bringing the two saline solutions in relation with pure water. Thus we find the excess of the capillary ascension of pure water over that of the liquid opposed to it, which is denser, and consequently rises the least, determines the force of the endosmosis, which latter is, therefore, a special result of the capillary force.

The only action of the difference in the density of the liquids is to produce difference of their capillary ascension, so that liquids of smaller density, which have a different capillary ascension, produce an endosmosis differing, and in proportion to the degree of their power of ascension: thus I have found that a solution of sulphate of soda, brought into relation with pure water, produced an endosmosis double of that which a solution of muriate of soda of the same density produced under the same circumstances. The cause of this is found in the measure of the capillary ascension of the two solutions. The capillary ascension of water in a glass tube being 12 lines, that of the solution of sulphate of soda, the density of which was 1·085, was 8 lines, while that of the solution of the muriate of soda, of the same density, was 10 lines. The excess of the capillary ascension of the water over that of the solution of sulphate of soda is 4, and over that of the muriate 2, or in the proportion of 2 to 1. Now this is precisely the proportion of that existing between the endosmosis produced in the experiment made by placing each of these solutions, of equal density, but different capillary ascen-

sion, in relation with the water. This phenomenon is, there unquestionably produced by the excess of capillary ascen of one of the liquids separated by the partition of the en mometer.

Hence endosmosis is the result of the opposition of unequal capillary forces acting at the two extremities o same capillary tube. These two forces impel the two opp liquids towards each other in unequal quantities, so that of them (that which has the smallest force of capillary as sion) is gradually augmented in volume; and it is this ex of capillary force which produces the endosmosis. Havin monstrated that the endosmosing liquid is impelled toward liquid with which it unites, in a quantity proportionate to excess of its capillary ascension over that of the liquid tow which it is so impelled, it remains to be explained by mechanism this phenomenon is produced. It appears ce that, under these circumstances, there are two opposite rents, the one strong and the other weak: these two curr may be seen by putting muriatic acid into a glass endosmo ter, and plunging it into a glass vessel filled with water. shall then see the water rise, forming striæ in the acid, and descend, forming similar striæ in the water. It canno supposed that these two opposite and unequal currents simultaneously through the same canals; but it is poss that each capillary canal serves alternately to transmit the opposing currents, and the following fact appears to prove be so. I put nitric acid into an endosmometer closed wi piece of bladder, and added to it some very small fragment gold leaf. I then plunged the apparatus into water. The dosmosis was immediately produced, and I perceived that water which it introduced into the acid raised rapidly some the fragments of gold leaf, whilst others remained station against the membrane. The fragments of gold leaf, which experienced an ascending impulse, fell again by their weight upon the surface of the membrane: they remai motionless there for an instant, and were then again stron impelled upwards: all the fragments of gold leaf presen these alternations of rapid ascension and fall, followed b short repose on the membrane, but without being at all sim

taneous. This fact proves evidently that the capillary canals, which transmit the water impelled towards the acid, or the acid impelled towards the water, do not contain a current constantly moving in the same direction. The fragments of gold-leaf, when they fall after their movement of ascent, are naturally directed towards the canals, which serve at the moment for the current descending from the acid towards the water; they remain motionless on this spot until the canals upon the orifices of which they are placed change their descending into an ascending current, when they resume their upward motion under the influence of the ascending liquid. Without these alternations of the two opposing currents in the same capillary canal, it is not easy to conceive how the endosmosis can be equal to the excess of the capillary action of the opposite liquid. In order to explain the phenomenon, we should have to admit either that the two opposing currents existed simultaneously in the same canal, which is impossible, or that the number of the capillary canals of the membrane is equally divided between the two liquids, and consequently between the two currents, and there appears no reason to suppose the existence of this equal division. But, as experience proves that the capillary canals do not transmit the same liquid without discontinuance, it becomes necessary to admit that the same canal serves alternately for the two opposite currents: this is the only manner in which we can understand the exact relative proportion which exists between the quantity of liquid which is accumulated and the excess of the capillary action of that liquid over that of its antagonist. If, in opposition to what appears to be proved by experience, we assume that there is but one current through the membrane, and that the opposite current is but an optical deception, the result of the affinity of mixture of the two liquids, then the effect of endosmosis would be explained by the opposition of two unequal forces of capillary impulsion; part of the greater force would be employed to counterbalance the smaller force, the effect of which it would thus suspend or neutralise, and there would only remain the excess of the greater force of capillary impulsion over the smaller force to produce endosmosis. In this view of the case, it would not be necessary to admit that the same capillary canal serves

alternately to transmit two opposite currents : all th
canals of the partition of the endosmometer would b
by currents flowing in the same direction.

Since the capillary action perfectly accounts for a
ditions of the phenomenon of the endosmosis, it
that the affinity of mixture of the liquids has no sh
production of this phenomenon; it exists, it is t
accessory phenomenon, but has no dynamic effe
miscible quality of the liquids be necessary to the
of the endosmosis, it by no means follows that the
acts as a dynamic cause of the effect produced. E
results from the opposed association of two unequa
actions. Now the capillary actions of oil and of
tually destroy each other; a tube, which has its int
rubbed over with oil, will not occasion the capillary
of water, so that there is not in this case any opposit
unequal capillary actions; there is but one, and
there is no endosmosis.

The *simple* capillary action, which alone has bee
known, is a force which never impels liquids beyond t
of capillary action; the *double* capillary action, whi
discovered, impels the two opposing liquids in oppo
tions across the capillary sphere of action, and tend
them out in unequal quantities on each side. All th
animal or vegetable membranes, which may be used
endosmometers, are eminently adapted to produce en
Among the mineral substances, which may be use
thin plates for the same purpose, baked clay is the bes
to produce this phenomenon; plates of carbonate
scarcely produce it at all; I was, indeed, disposed t
that the latter substance was totally incapable of pro
but, on repeating my experiments, I succeeded in
a very feeble, but still perceptible endosmosis with a
white marble of the thickness of one millimetre.

All the liquids which are susceptible of a chemical
nation, with the permeable partition of an endosmome
pend the endosmosis, after having produced it for a l
shorter period. Thus, when an acid, alkaline, or sali
tion is placed in an endosmometer, an endosmosis i

produced, but after some time this effect ceases, and the liquid raised above its level has a tendency to fall by filtration through the permeable partition. This occurs when the partition is modified by the acid, the alkali, or the salt. Alcohol produces the same effect, which results with extreme promptitude from the action of sulphuric and hydro-sulphuric acids, so much so, indeed, as to induce me to believe that these two acids are opposed to the endosmosis. The organic liquids, not having any chemical action on the membranes, or mineral partitions which may be employed to close the endosmometers, produce an endosmosis which would not be liable to any suspension if they always remained the same. But these liquids are decomposed and become acid or alkaline, and often become charged with sulphuretted hydrogen. From that moment, their endosmosing action becomes liable to be destroyed. Thus a membrane acidified, salified, or alkalised, as much as it can be by the acid, saline, or alkaline liquid with which it is in contact, will no longer produce any endosmosis with the same liquid. In order to obtain an endosmosis, which will not cease spontaneously, a permeable partition must be placed in contact, on one side with pure water, and on the other with a liquid which has no chemical action on it. Thus if we put a solution of muriate of soda into an endosmometer closed with a plate of clay, an endosmosis will be produced which will not cease. But if the partition of the endosmometer were membranous, the endosmosis would cease as soon as the partition became salified. Sulphuretted hydrogen puts an end to the endosmosis produced with a plate of clay, as well as with a membrane; because, in both cases, it combines with the elements of the permeable partition. The extreme rapidity of this combination is the cause of the rapid manner in which that substance puts an end to the endosmosis. The same effect is produced by sulphuric acid; but I repeat, that it will also be produced by all liquid substances, which are susceptible of combination with the elements of the permeable partitions of the endosmometer; the only difference in that respect is in the rapidity of the combination. It is not easy to determine with exactness the physical causes of these latter phenomena; but it is evident that they depend upon the capillary action which is modified in the permeable partitions by the chemical

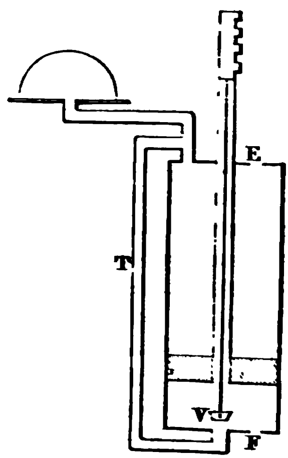
modifications sustained by the latter; and in fact, it known, that the capillary action experiences as much va in its effects from solids of different natures as from d liquids.

ON A DOUBLE-ACTING AIR-PUMP.

By the Rev. WILLIAM RITCHIE, M.A., F.R.S.,
Prof. of Nat. and Exper. Philos. Royal Institution.

SEVERAL attempts have been made to construct acting air-pumps; but from the fact that none of th in use, we may conclude that the practical difficulties att the construction of them were too great to bring them i petition with those in common use. The following conti appears to me sufficiently simple, and will obviously the power without adding materially to the expense, an very little additional friction.

It consists of a barrel similar to Smeaton's air-pump, a solid piston, with a piston-rod working air-tight in a c leather. The piston-rod has a hole drilled along it



the whole length of the barrel, for the purpose of ing a brass rod, about the ¼th of an inch in diameter. upper end of the rod is slit about an inch, and slightly o so as to act as a spring by its friction in raising and dep

the lower valve. To the lower end of the rod is fixed the conical metallic valve V, which is allowed to rise and fall about the tenth of an inch. A bent tube, T, connects the upper and lower divisions of the barrel formed by the solid piston. This tube is continued from the top to the plate of the air-pump. At the entrance of this tube into the upper end of the barrel, is placed a valve of oiled silk, opening inwards, to allow the air from the receiver to expand into the upper part of the barrel when the piston is depressed. Two valves, either conical or of oiled silk, are placed on the upper and lower ends of the barrel at F, E opening outwards, to allow the air in the barrel to escape into the atmosphere. When the piston is depressed, the conical valve shuts the communication between the barrel and the receiver, and the air is forced out at the valve F, whilst the air in the receiver rushes into the space above the piston to supply the vacuum thus formed. When the piston begins to rise, the conical valve, on the end of the brass rod, is raised, and the air from the receiver follows the piston till it has reached the top of the barrel, and expelled the air through the valve E. The next depression of the piston performs a similar office, and thus the full of the barrel of air, of the same density as that in the receiver, is at each stroke expelled, and consequently the exhaustion will go on with twice the rapidity of that produced by a single barrelled air-pump of the same size.

Instead of the hollow tube for the piston-rod the wire might be made to pass through the piston, as in the French construction, with two conical valves on the extremities; but the construction I have described seems to me the least liable to objection.

ON THE METHOD OF OBSERVING THE FIXED LINES IN THE SOLAR SPECTRUM.

By J. T. COOPER, Esq.

AS many with whom I am acquainted have sought in vain for those dark bands which occur in the solar spectrum produced by prismatic refraction, usually known by the appel-

lation of Fraunhofer's lines; and as I have never work, the precise method described that is nec employed for their successful production, it occ that a description of the instrumental means I not be unacceptable to some of your readers, and of them who have more opportunity than myself, a rous of pursuing the investigation into the cause c duction, to proceed in their researches without the consequent on the experiments of any kind, wher has to be sought. It is, therefore, with this view written, and endeavoured, if possible, to save a p of time and expense to those who may feel inclin this rich field of inquiry, and to enable those wh to determine the refractive and dispersive powers of to do so with precision; and, should you concur the propriety of these suggestions, to request you to insertion in your useful publication.

In article 422 of Mr. Herschel's admirable pape in the Encyclopædia Metropolitana, he says that ' prisms of our manufacture it would be quite useless the experiment.' An assertion coming from such h rity is of itself sufficient to deter any one from n trial; and he recommends the substitution of hollo filled with highly refractive media, in lieu of the gl With this assertion I am in some degree disposed to but certainly not to the extent he there intimates. that not one prism in twenty is fit to be employed fo pose; yet I have obtained several of British glass, bo plate, and crown, and of various refracting angles, shown not only the most prominent of the lines, but e that may be considered as of the second and third ord such abundance, under favourable circumstances, tha be no easy matter to count them: suffice it to say, th prism is necessary, and such can be met with, though out some difficulty; but neither its size, as respects it the breadth of its sides, the refracting angle, nor th glass of which it is made is a matter of much mome

* I have in my possession equilateral prisms of flint, plate, and c which are only three-quarters of an inch long, and the sides less than t

the preference is to be given to one of flint glass with a large refracting angle (say 50° or 60°), because of its high refractive and dispersive powers; all that is required is, that it should approach as near to perfect homogeneity as possible. A few streaks running parallel to the refracting edges may be disregarded; but if they are thrown into waves or curved lines, by injudicious workmanship, the prism is utterly useless for this purpose. Such a prism being obtained, let it be placed twenty, thirty, or more feet distant from, and with its refracting angle parallel to a very narrow linear opening in the window shutter of a darkened room: such an aperture or opening may be conveniently formed in a piece of tin foil, or thin sheet lead, by the point of a sharp penknife; and means must be taken to illuminate this aperture either by the rays of the sun reflected from the surface of a plane mirror, or by the light from a bright sky: the former is decidedly to be preferred; that part of the solar miscroscope which is employed for a similar purpose when that instrument is used, will be found very convenient for this. The prism being placed in the beam of light transmitted by the aperture, either in a vertical or horizontal position, and its refracting angle as nearly parallel as possible to the aperture, turn it round on its axis until the refracted spectrum of the illuminated aperture formed by the prism appears to be stationary; the prism is then in the position of minimum deviation, and the most favourable one for the production of the lines.

If the spectrum be now carefully examined, a number of narrow black stripes will be seen crossing it at various distances from each other; but as there is some difficulty in seeing them with the unassisted eye, on account of their minuteness, it is better to employ a telescope for the purpose, which, however, need not be of large dimensions, nor possess very high magnifying power; the one I generally employ for this and similar purposes is 1·6 inch aperture, and 18·3 inches focal length, mounted on the common portable stand, and has amplifying

broad, with which I have seen the lines very distinctly; also in a small triangular prism of rock crystal, with its refracting angles parallel to the axis of the original prism, I have seen the lines both in the ordinary and extraordinary spectrum of the aperture perfectly sharp and well-defined.

powers from 15 to 130 times, the object-glass of w
placed as near as may be convenient to the p
power of 40 or 50 times selected for the occasion;
ever, the adjustment of the focal length of the
made to see the spectrum perfectly sharp and d
edges, the dark lines will be seldom seen, or, if se
very faint and indistinct; but if the drawer, or
the telescope be pushed in about half an inch, t
be seen to perfection; and by a nice adjustment,
the major part of the lines nearly as sharp and we
the spider lines in the micrometer, employed for t
ment of their distances from each other.

Lambeth, 8*th October*, 1831.

ON THE ACOUSTIC FIGURES OF PLAT

By Professor Strehlke.

IN the second Number of the 8vo. vol. of Gilbert
Professor Strehlke communicated some very int
periments on this subject, which led him to conclud

1. *That acoustic figures are composed of curves*
2. *That these curves do not intersect each othe*

And as Chladni had expressed some doubts on th
of the experiments, (chiefly on account of their h
made with metallic plates,) Professor Strehlke
repeated them on glass plates, and convinced hin
correctness of his former experiments, and of the
which he was led by them. The difference between
on metallic and on glass plates, says Professor Strel
speaking of his late experiments, is very trifling; anc
periments are made with sufficient accuracy, the at
will be equally proved by the figures on either, bu
are much more distinct on ground-glass plates, meta
or glass plates covered with leaf gold, than on
polished glass; and when he used a plate of the la
but covered on one side with leaf gold, the distinc

tween the two figures was very different, so much so that whilst, on the one surface, the curves evidently came only near each other, it was doubtful whether the lines on the other surface were intersected or not. Great attention is further to be paid to other circumstances which might influence the experiment, for the least change of temperature, a very slight degree of humidity, the commencement of oxidation, or any external agitation in the vicinity of the plate, is sufficient to disturb the formation of distinct figures. The quantity of sand* is also of great importance in that respect, and ought not to exceed above three or four grains on a square line; the bow must be moved up and down steadily, and until the figure has ceased to undergo any further change; and if the figures are to be measured, it is indispensable to reproduce them as long as they are not formed by one row of grains only, the joint central line through which may then be regarded as the quiescent line; for if there were several grains, three, for instance, we should by no means be justified in considering the middle one as the representative of the quiescent line, as either of the two others might in reality stand for it; for it is as probable that one of the outer grains is balanced by the two others, as that the middle one is fixed to the two outer grains, &c., for that the plate rests in mathematical lines is, we believe, universally admitted; and although sometimes, if much sand be used, broad lines are formed, the outer rows may always be seen to move as long as the plate sounds, whilst one line is completely quiescent.

In the following experiments the plates were supported on one side only, either on a vertical wooden bar with a small piece of cloth at the point of contact, or merely on the spread fingers of the left hand; either of these two methods is preferable to the use of the screw, which scarcely admits of the reproduction of the same figure; for the least difference in the tension or in the plane where the plate is fixed, changes the figure, &c., which is not the case if the plate is supported in the manner above described, where it is sufficient that the

* Professor Strehlke always used sea-sand, the grains of which, under the microscope, appeared as spheroids from $0'''.03$ to $0'''.05$ diameter.

resting point be nearly the same, in order to pro
figures.

The lines were measured by a scale of sufficien
convince Professor Strehlke that the results were
$\frac{1}{20}$th of a line.

One of the most simple acoustic figures, fig.
when three or four corners of the plate are su
the bow is drawn at the middle of one of the side
obtained by simply holding the plate with the
placing the thumb on E, fig. 1, the first finger
the little finger on D, and then drawing the bov

Fig. 1.

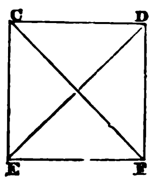


Fig. 2.

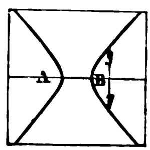
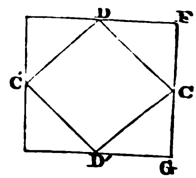


Fig. 3.



middle of DF. On all regular plates of metal o
figure will be found to be a hyperbola, the angles be
principal axis and asymptotes of which exceed 4
direction of the principal axis cannot be determined I
to the experiment; but after it is found, it will be l
mark it down on the plate, as the direction of a gr
other figures is dependent on it.

On three plates, two of brass and one of copper, t
nates were equally divided by the axis; B was take
commencement of the co-ordinates, and on the sup
that the curve was a conic section of the equation

$$y^2 = px + qx^2;$$

the most probable value of the coefficients was determ
the method of the least squares.

In a square plate of brass (No. 1.) of 34.3 Parisian l
length, and 1'''.1 thick, the equation deduced from the
sured value of the co-ordinates was

$$y^2 = 10836 \,.\, x\,(x + 6'''.27),$$

and the relation between the observed and calculated values of y was

Observed.		Calculated.	Difference.
x	y	y	$'''$
1·05	2·94	2·89	+ 0·05
1·72	3·88	3·86	+ 0·02
2·99	5·43	5·48	— 0·05
4·43	7·15	7·18	— 0·03
5·71	8·64	8·61	+ 0·03
9·20	12·41	12·42	— 0·01

So that the results of calculation and observation may almost be considered to accord.

The calculated distance between the summits of the curve, $= 6'''{\cdot}27$, was also found to agree with observation, it being in three different experiments, $6'''{\cdot}25$ $6'''{\cdot}26$, and $6'''{\cdot}24$.

The plate was now ground, in order to see what change this would produce in the situation of the curve. The following equation was obtained:

$$y^2 = 1{\cdot}1032 \,.\, x \,.\, (x + 6'''{\cdot}228)\,;$$

and the difference between measurement and calculation was,

Observed.		Calculated.	Difference.
x	y	y	$'''$
0·06	2·33	2·24	+ 0·09
1·39	3·46	3·42	+ 0·04
2·16	4·52	4·47	+ 0·05
2·88	5·46	5·38	+ 0·08
3·77	6·37	6·45	— 0·08
4·60	7·45	7·41	+ 0·04
6·29	9·25	9·32	— 0·07
8·75	12·05	12·03	+ 0·02

so that scarcely any alteration had been produced in the curves.

Both branches of the hyperbola appeared to be equal; this was however only apparent, for when it was taken as the origin of the co-ordinates, it was found that

$$y^2 = 1{\cdot}1627 \,.\, x \,.\, (x + 5'''{\cdot}379),$$

and the unit of measurement and calculation to be

Observed.		Calculated.	Differen
x	y	y	'''
0·78	2·30	2·36	— 0·0
1·39	3·43	3·31	+ 0·1
2·22	4·38	4·43	— 0·05
3·02	5·46	5·43	+ 0·03
3·74	6·34	6·30	+ 0·04
4·60	7·20	7·31	— 0·11
5·15	7·98	7·94	+ 0·04
8·48	11·69	11·69	+ 0·00

by which it appears that the elasticity of the on plate slightly differed from that of the other.

In another brass plate (No. 2.) of 34'''·3 length, thick, the equation for the branch B of the hyperbo

$$y^2 = 1·1421 . x . (x + 6'''·126) \quad . \quad . \quad . \quad . \quad (\ldots$$

and that for A

$$y^2 = 1·1472 . x . (x + 6'''·062) \quad . \quad . \quad . \quad . \quad (2$$

which shows that the elasticity in both halves w equal.

The difference between measurement and calculatio from the following tables:

(1)

Observed.		Calculated.	Difference.
x	y	y	'''
0·39	1·66	1·70	− 0·04
1·44	3·60	3·53	+ 0·07
2·52	4·99	4·99	+ 0·00
3·71	6·40	6·46	− 0·06
4·82	7·76	7·76	+ 0·00
5·90	8·97	9·00	− 0·03
6·87	10·16	10·10	+ 0·06
9·86	13·41	13·42	− 0·01

(2)

Observed.		Calculated.	Difference.
x	y	y	'''
1·16	3·19	3·10	+ 0·09
2·10	4·57	4·43	+ 0·14
3·10	5·65	5·71	− 0·06
3·99	6·81	6·78	+ 0·03
4·99	7·92	7·95	− 0·03
5·87	8·92	8·96	− 0·04
7·04	10·30	10·29	+ 0·01
9·14	12·63	12·62	+ 0·01

In a square plate of copper (No. 3.) of $34'''.3$ in length, and $1'''.33$, the equation for B was found to be

$$y^2 = 1027 \cdot x \cdot (x + 3'''\cdot 846),$$

and the distance between the two summits $3'''\cdot 88$; the results of observation and calculation agree in the following manner:

Observed.		Calculated.	Difference.
x	y	y	'''
0·51	1·44	1·51	− 0·07
0·79	1·83	1·94	− 0·11
1·33	2·60	2·66	− 0·06
1·62	3·05	3·02	+ 0·03
2·48	3·99	4·01	− 0·02
3·10	4·93	4·91	+ 0·02
4·12	5·82	5·81	+ 0·01
5·04	6·65	6·78	− 0·13
5·76	7·53	7·54	− 0·01
6·70	8·59	8·52	+ 0·07
7·70	9·64	9·55	+ 0·09
8·75	10·54	10·64	− 0·10
9·92	11·80	11·84	− 0·04

In order to ascertain whether the distance of the summits was in any way related to the length of the side of the plate, this distance was measured on a great number of plates, and then compared with the length of the side. In the following table $2a$ is the distance in decimal fractions, the entire length of the side being 1·000, and φ the angle between the asymptote of the hyperbola, and the principal axis.

	$2a$	φ
Brass plate, No. 1.	0·181	46°·9′
Ditto ditto, No. 2.	0·177	46·56
Ditto ditto, similar to No. 2.	0·116	45·46
Ditto ditto, of 53'''·6 in length	0·207	46·36
Ditto ditto, of 57'''·8 in length	0·151	46·13
Copper plate, No. 3.	0·112	45·23
Two copper plates similar to	0·263	47·36
the brass plate No. 2.	0·326	49·46
Zinc plate of 53'''·6	0·175	46·8
Ditto ditto 34'''·9	0·189	46·18
Ditto ditto ditto	0·167	46·25
Tin plate of ditto	0·164	46·12
Plate of bronze	0·155	45·47
Glass plate covered with a thin layer of sealing-wax	0·179	46·37
Ditto ditto with a solution of gum in alcohol	0·174	46·30
Ditto ditto ditto ditto sulphur, æther	0·127	45·28

These observations do not seem to lead to any certi except that if $2a$ increases, φ increases also, in ac with what is observed in plates which are fixed betwee where the distance between A and B may be inc *libitum*, by fixing the plate at a greater or less distanc middle. Thus, if the brass plate, No. 2, (in which 2(and $\varphi = 46°{\cdot}56'$) was fixed between screws at a poi principal axis to the right from B, $2a$ was 0.: $\varphi = 51°{\cdot}53'$; if to the left from B, $2a$ was 0·148, and φ If the plate was fixed at any other point the tone rem same, but the axis of the hyperbola was perpendicu: of the former figure.

Another very simple acoustic figure is obtained by ing the sides D C D′ (fig. 3) at the middle, and the vibration at one of the corners; it consists of tv bolic branches, the principal axis of which coincide diagonal of the square: sometimes fig. 4 is prod major axis of which is always in the direction of the axis of the hyperbola in fig. 2.

The formation of this figure seems to be fully acc(by the theory of undulations, for in a vessel of the s as the plate, and filled with liquid, the interferenc undulations will produce the square C D C′ D′, the resistance which the undulations experience in tions C C′, and D D′, is equal; if not, and if the re: the direction D D′, is greater than to that of C C′, of the square will be changed into hyperbolic co the longer axis of the ellipse will be in the direction the shorter in that of D D′; and if further, the in the same direction is unequal, the figure will be(gular with regard to its opposite parts.

The difference of the two axes in fig. 4, is propo the distance between the summits of the hyperbola in

On the brass plate No. 1, C C′ was	$= 30'''{\cdot}58$	D D′	$= 27'''{\cdot}92$, and
Ditto ditto No. 2, „	= 31 ·36	„	= 28 ·03 ,
On a brass plate of the same dimension	= 30 ·86	„	= 29 ·36 ,
On the copper plate No. 3, „	= 29 ·53	„	= 29 ·03 ,

On two copper plates, on which A B had been f(

0·263 and 0·326, (the side of the plate being 1·000,) D D′ was 25‴·2 and 26‴6, the curves had the form of fig. 5, C and C′ being beyond the plate.

Fig. 5.

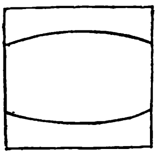


Fig. 6.

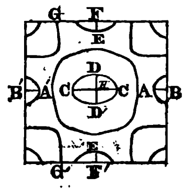


Fig. 7.

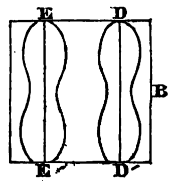
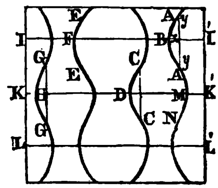


Fig. 8.



The curve D C D′ E′ was also measured and calculated on the brass plate No. 1. The co-ordinates on C C′ were measured, and for the curve A C B the fourth part of D C D′ C′, the equation was found to be

$$y^2 = 0·4663 \,.\, x \,.\, (x + 25'''·829),$$

and the results of observation and calculation agreed in the following manner:—

Observed.		Calculated.	Difference.
x	y	y	‴
0·78	3·10	3·10	− 0·01
1·27	4·76	4·77	− 0·01
2·41	5·65	5·63	+ 0·02
3·16	6·54	6·53	+ 0·01
3·79	7·31	7·23	+ 0·08
4·54	7·92	8·02	− 0·10
5·21	8·78	8·68	+ 0·10
5·87	9·28	9·31	− 0·03

For A′ C′ B′ the equation was

$$y = 0·4434 \,.\, x \,.\, (x + 26'''·777),$$

with the following result of observation and calculation,—

Observed.		Calculated.	Difference.
x	y	y	‴
0·50	2·38	2·06	− 0·08
1·33	3·99	4·07	− 0·08
2·11	5·21	5·20	+ 0·01
2·88	6·20	6·15	+ 0·05
3·66	6·98	7·03	− 0·05
4·43	7·87	7·83	+ 0·04
5·16	8·64	8·64	+ 0·00
6·09	9·42	9·42	+ 0·00

In A D A′ it was necessary to incline D D′ toward order to divide the ordinate equally, the co-ordinates sequently not quite perpendicular. It was found tha

$$y^2 = 0{\cdot}4989 \, . \, x \, . \, (x + 33'''{\cdot}005),$$

from which y was calculated with the following result

Observed.		Calculated.	Difference.
x	y	y	′′′
0·50	2·77	2·92	− 0·15
1·44	5·07	5·02	+ 0·05
2·33	6·40	6·46	− 0·06
3·24	7·76	7·72	+ 0·04
4·02	8·67	8·69	− 0·02
4·90	9·69	9·70	− 0·01

In the curve B D′ B′, the co-ordinates were perf pendicular, and the equation deduced was

$$y^2 = 1{\cdot}001 \, . \, x \, (x + 15'''{\cdot}14),$$

which shows a very great difference of elasticity o sides of the plate; the centre of the plate was also 0′′′·6 nearer to D′ than to D.

In the same curve, on the brass plate No. 2, the for A C B was found to be

$$y^2 = 0{\cdot}445 \, . \, x \, (x + 24'''{\cdot}459),$$

and for A′ C′ B′

$$y^2 = 0{\cdot}4002 \, . \, x \, (x + 28'''{\cdot}597);$$

where the results of calculation and observation found to correspond.

The other acoustic figures consist merely of a rep the above curves. Fig. 8 consists of four squares which contains Fig. 2, and will be obtained by sup plate at F and D′ (Fig. 3), or C and E (Fig. 1), at of any of the four squares. The direction of the axes will be parallel to that of A B in Fig. 2; the a of the inner hyperbolas become also hyperbolic curv the whole figure might be constructed *à priori*, pro elasticity is perfectly equal; if not, some of the h will have their axes perpendicular to the direction Fig. 2.

The measurement of the curves was made on a squ plate, 53′′′·63 in length, and 0′′′·7 thick. The line F

is parallel to the side of the square, and divides the ordinates into two equal parts, was at the distance of almost the fourth part of the side from the edge: the equation for A B A was

$$y^2 = 1{\cdot}643 \,.\, x\,(x + 5'''{\cdot}554)\,;$$

and for E F E

$$y^2 = 1{\cdot}533 \,.\, x\,(x + 5'''{\cdot}591)\,;$$

and the results of observation and calculation were found to correspond in the following manner:

For A B A,

Observed.		Calculated.	Difference.
x	y	y	'''
0·89	2·99	3·07	− 0·08
1·72	4·49	4·53	− 0·04
2·55	5·93	5·83	+ 0·10
3·55	7·15	7·28	− 0·13
4·33	8·56	8·45	+ 0·11
5·32	9·09	9·75	− 0·06
5·98	10·66	10·64	+ 0·02

For E F E,

Observed.		Calculated.	Difference.
x	y	y	'''
0·94	3·10	3·07	+ 0·03
1·83	4·49	4·56	− 0·07
2·71	5·98	5·87	+ 0·11
3·71	7·20	7·27	− 0·07
4·71	8·64	8·62	+ 0·02

For C D C the equation was

$$y^2 = 1{\cdot}676 \,.\, x\,(x + 4'''{\cdot}662)\,;$$

and for G H G

$$y^2 = 0{\cdot}9213 \,.\, x\,(x + 11'''{\cdot}034),$$

with the following results:—

For C D C,

Observed.		Calculated.	Difference.
x	y	y	'''
0·28	1·39	1·52	− 0·13
0·55	2·16	2·19	− 0·03
1·05	3·21	3·17	+ 0·04
1·33	3·71	3·65	+ 0·06
1·94	4·50	4·63	− 0·07
2·22	5·10	5·06	− 0·04

For G H G,

Observed.		Calculated.	Difference.
x	y	y	'''
0·33	1·99	1·86	+ 0·13
0·83	2·99	3·01	− 0·02
1·27	3·77	3·79	− 0·02
1·72	4·52	4·50	+ 0·02
2·11	4·99	5·05	− 0·06
2·55	5·65	5·65	+ 0·00
3·21	6·34	6·49	+ 0·05
3·77	7·15	7.17	− 0·02

The equation $y^2 = 1{\cdot}479 \,.\, x\,(x + 5'''{\cdot}44)$, is the sion of the curve A M N.

The whole figure is consequently composed of hype different form, according to the different degree of ela the various parts of the plate.

It may be easily anticipated what figure will result division of the square into nine smaller ones, &c.

If the plate being supported at D and D′ (Fig. 3 the middle of F C or C G, the vibration is produc Figs. 9 and 10 will be formed, in which A B is con

Fig. 9.

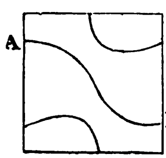


Fig. 10.

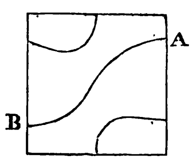
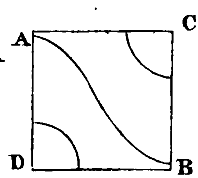
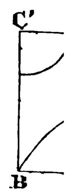


Fig. 11.





two hyperbolas meeting at the centre of the plate A and B (Figs. 11 and 12), and a point, which is dis C or C′ by a third of C B, are supported, Figs. 11 a be obtained, which differs from Figs. 9 and 10 only A B comes nearer a straight line, and that the lin corners are almost parts of circles. The tone is for same, but that of Figs. 11 and 12 is the fullest of the

If the plate, instead of being supported, is fixed between a screw at a point distant about one fourth of the side from it, and half of the side from the edges, a remarkable transversion of the Figs. 9 and 10 take place. Suppose two lines drawn through the above point, parallel to the sides of the square, and the four right angles to be 1, 2, 3, and 4: if the plate is fixed in angle 1, at the distance of a few lines from the point of intersection, Fig. 9 will be formed; if in angle 2, it will be Fig. 10; in angle 3, Fig. 9; and in angle 4, Fig. 10 again: this is always the case, and it is not even necessary that the point where the plate is fixed should be always exactly at the same distance from the point of intersection; the distance of the curves from each other only will be changed.

The measurement of Fig. 12, on the copper plate No. 3, led to the following result: *e* was considered as the commencement of x and y, and the equation of the curve on the copper plate No. 3 was

$$y^2 = 0{\cdot}8904 \,.\, (143{\cdot}2 - x^2);$$

the calculation of y from which was found pretty nearly to correspond with the result of observation.

On the brass plate No. 1, the expression of the curve was

$$y^2 = 0{\cdot}899 \,.\, (144{\cdot}8 - x^2),$$

and on the brass plate No. 2,

$$y^2 = 0{\cdot}8962 \,.\, (334{\cdot}58 - x^2).$$

If half of the greater axis of the ellipse is made $= a$, and half of the lesser axis $= b$, the length of the side of the plate being $= 1{\cdot}000$, it was found that

on the copper plate a, was	$= 0{\cdot}348$ and b	$=$	$0{\cdot}328$
on the brass plate No. 1	$= 0{\cdot}350$	$=$	$\cdot 332$
and on the brass plate No. 2	$= 0{\cdot}341$	$=$	$\cdot 323$

or that, in general, the lesser axis of the ellipse is nearly one sixth of the length of the side.

If the plate is divided into four squares, Figs. 11 and 12 being repeated four times, will produce Fig. 13 or 14.

Fig. 13 is obtained when the plate is supported at A B, and at a third point, distant from any corner by one third of the

side, whilst the vibration is produced at the middle
It consists of the two branches of a hyperbola, th

Fig. 13.

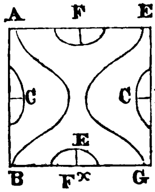


Fig. 14.

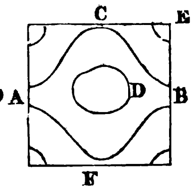
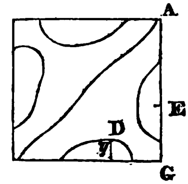


Fig. 15.



axis of which is in the direction of A B in Fig. 2, ellipse F F′ and D D′, of which D and D′ are sm F and F′.

On the brass plate No. 2, F′ and *e* were taken as mencement of the co-ordinates, and the equations f ellipses were

$$y^2 = 5{\cdot}3477 + 1{\cdot}6366 \,.\, x - 0{\cdot}6725\, x^2$$

and $$y^2 = 2{\cdot}8798 \,.\, x\,(11'''{\cdot}01 - x).$$

Fig. 14 is formed by supporting the plate at A (that D B = $\frac{1}{3}$ A B ; and by producing the vibration long axis of A C B F, and of the central ellipse, is in direction as the principal axis of the hyperbola in Fi the brass plate No. 2, the figure was not quite A and B.

Figs. 11 and 12 may further be transformed into Fi 16: if the plate is supported at A B, and at a po diagonal perpendicular on A B, at the distance of $\frac{1}{8}$ S, and the vibration is produced at E, E S being In either case the ellipses of Figs. 11 and 12 see changed into hyperbolas, the principal axis of which perpendicular to the diagonal A B (Fig. 15), or par The equation for D C (Fig. 15) was found to be

$$y^2 = 80{\cdot}546 \,.\, 1{\cdot}7848\, x - 0{\cdot}9485\, x^2.$$

Fig. 6, which will be easily recognised as the repet more simple figure, is produced when the plate is sup

S S′ and A (or, instead of A, at A E or E′), and caused to vibrate at B. The long diameter C C′ of D C D′ C′, as may be anticipated from the above, is parallel to the principal axis of the hyperbolas in Fig. 2; and this is also the case with the lateral ellipse B B′ F F′. But if, instead of supporting the plate, it is fixed between a screw, at a point of F E, which is nearer to the centre than E, the long diameters of the curves are in the direction of D D′, and the other parts of the figure are changed accordingly.

If, in Fig. 6, the plate is supported at S S′, and at the middle of H B, Fig. 7 is formed, in which E E′ and D D′ are always in the direction of the principal axis in the hyberbola of Fig. 2.

EXPERIMENTAL ILLUSTRATION OF THE EQUALITY BETWEEN THE RADIATING AND ABSORBING POWERS OF THE SAME SURFACE.

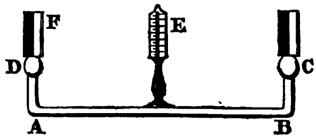
By the Rev. WILLIAM RITCHIE, M.A., F.R.S.,
Prof. of Nat. and Exper. Philos., Royal Institution.

PROFESSOR Leslie has shown, by a series of ingenious experiments, that those surfaces which radiate heat most copiously also absorb it in the greatest quantity; but, as far as I know, it has not been experimentally demonstrated, at least in a manner adapted to illustration before a large audience, that the radiating and absorbing powers are exactly equal to each other. The simplicity of the following mode of illustration, and the clear conviction which it brings to the minds of an audience, have induced me to give it a place in the Journal of the Royal Institution.

The instrument consists of a large differential thermometer, with cylindrical chambers made of the thinnest tin-plate, similar to what I formerly described in the transactions of the Royal Society *. The horizontal branch, A B, of the glass tube is

* Phil. Trans., 1827, p. 123.

about a foot long; the vertical portions, AD, BC, l



four or five inches. Near the extremities of the ve tions two small bulbs are blown, for the purpose of part of the coloured liquid and preventing it entering drical chambers. The cylindrical chambers may b four inches in diameter and half an inch thick, havin soldered in the rim for receiving the extremities of tube, which is rendered air-tight by common electr The surface of one of the cylindrical chambers is c lamp-black, whilst the opposite surface of the othe is left perfectly bright. To the glass tube is attach scale, divided into any number of equal parts. T which I prefer, on account of the delicacy of its n spirit of wine, tinged in the usual way.

A cylinder of tin-plate, of the same diameter as bers of the thermometer and about an inch thick, h of its sides coated with lamp-black and the other lef to be placed exactly half way between the chambers filled with hot water, when the following phenome observed:—

1. When the coated side of the cylinder D is turne the coated side of the chamber D′, the fluid in the ste sink with extreme rapidity. The reason of this is obvic the coated side of the cylindrical canister we have an quantity of radiant heat shooting out at right ang surface, and falling on the powerfully absorbing surf cylindrical chamber of the instrument; whereas, opposite metallic surface of the canister we have a v portion of radiant heat, and that, too, falling on which absorbs only a minute portion; hence the stri rence between the temperature of the air in the two of the thermometer.

2. Remove the canister, and again place it exac

middle between the two chambers as before, with its coated side turned towards the bright side of the cylindrical chamber and its bright side towards the coated surface of the other chamber, fill it with hot water, and the liquid in the stem will be found to remain *perfectly stationary*. The reason of this beautiful result is equally obvious: From the coated surface of the canister we have a copious flow of radiant heat,—suppose ten times as much as from the metallic surface,—which falls on a surface of feeble absorbing power, which we shall suppose takes in only one part out of ten of the whole radiant stream; from the other side of the canister we have only a very small portion of radiant heat, which we suppose equal to one, which portion is absorbed by the coated surface of the chamber, and rapidly conveyed to the included air; and, since the effect on both chambers is the same, we conclude the radiating and absorbing powers are exactly equal to each other.

If the surfaces be scratched or coated with other substances, the same law will be found uniformly to obtain. The instrument which I have described is not only useful for the striking illustration of this fact in the lecture-room, but, with slight variations, may be successfully employed for illustrating the whole theory of radiant heat.

ON THE PENETRATIVENESS OF FLUIDS.

By J. K. MITCHELL, M.D., Lecturer on Medical Chemistry in the Philadelphia Medical Institute.

[Continued from page 118.]

Having completed the *first* series of experiments on molecular infiltration, before entering upon an account of the second, reserved for the next number of the Journal, it may be refreshing both to experimenter and reader, in a very toilsome investigation, to pass in cursory review some of the almost infinite theoretic and practical suggestions, which flow from the facts before us.

The most striking generality, is that of the *high power* of penetrativeness of *gases* for *organic molecular tissue*, long known to be infiltrable by liquids, but until now not generally known to admit of any permeation, by at least, *insoluble* aëriform substances.

Secondly. We are struck with an unexpected resul
POWER of gases to infiltrate *each other*. It has been k
that aëriform substances confined in the same apartm
mingle uniformly, and that, even if the lighter one be pl
the other. To account for this, and some other facts o
class, Mr. Dalton supposed that each gas, in reference t
cal relation of its particles, stood in an attitude of inde
any other gas present, as much as if no such gas were con
with it, no particle of one gas being supposed to rest o
cle of the other, the interstitial cavities of one gas bein
vacuum for the reception of the molecules of the othe
each.

The *power*, however, of this infiltration being kno
entitled to conclude, that the interspaces of gases are
occupied with a force similar, and probably equal to
causes the imbibition of liquids by solids, and produces
substances, even of the highest cohesive attraction. Sol
now be esteemed infiltrations by solids and liquids of th
each other, *requiring*, perhaps, only a *fitness in size*, ra
chemical or cohesive attraction, for we see it subvertin
greatest cohesive power, and holding no apparent rel
known chemical affinities.

The atmosphere cannot any longer be considered as a
the common acceptation of the term. Its gases pene
other interstitially with great mechanical force, so great
all mere mechanical means of separation. It is an exempl
solution.

When the particles of a solid separate and enter the
liquid, it is termed solution, when the liquid penetrates
and the latter maintains its solidness, it is usually called i
imbibition, absorption, &c. &c. The processes are perfec
principle, the different names being expressive of that, anc
accompaniments or effects also.

By means of our second generality, we are enabled sa
to explain many phenomena not heretofore easily acco
Thus we understand how a gas or odour flows so rapid
the whole tissue of a still atmosphere, and why some ga
more speedily than others. An explanation is also giv
diffusion of odours, even against a draught, or curre
accounts for this fact, among others, that brimstone throw
fire, is perceived by the olfactories, when the draught of th
is even perfect.

As proved in some experiments, already detailed, many solids are dependent on water for the power of penetrating tissues, or gases, &c., and it appears probable that many odorous solids, in particular, enter the atmosphere, solely by penetrating its hygrometric constituent. Thus, in solution, colouring matters readily, in certain cases, pass through membranes impenetrable without such aid, and every one has perceived the singular smell of a dusty road, after a shower, even at a very considerable distance. In a damp day, or immediately after rain, we more distinctly and vividly enjoy the fragrance of the parterre. Malaria seems to be dependent on the same cause for its penetration into the atmosphere, for every one knows the greater hazard of a residence in low damp situations, and the general unhealthiness of a damp summer, or autumn. As electricity is a great hydragogue, and substances in a negative state forcibly attract moisture, we might expect to find that season most damp and unwholesome in which the atmosphere maintained an electro-negative condition, and that driest and most healthful when it was electro-positive. Facts on this subject are yet to be created; but this one presents an aspect germain to the subject. Mr. William Mason, of Philadelphia, a philosophical instrument-maker, respected both for his ingenuity and correct moral character, informed me, that when, in 1820, the yellow fever existed here as an epidemic, he could not excite an electrical machine at his residence, in the infected district, although at his shop, which lay at some distance from it, the operation of the machine was sufficiently powerful*.

There exists between the lower surface of air, and the upper surface of water, a space possessed of powers analogous to those of the interspaces of substances in general. Along this plane, certain sub-

* Aqueous gas penetrates the air more or less rapidly according to the temperature and moisture of the atmosphere. According to our law of *progressive diminution*, evaporation is slower in a moister atmosphere, and *vice versa*. The following experiment shows that aqueous gas has also *its rate* of penetration. A long tube, surmounted by a bladder, held water and mercury; the former of which being above, was in contact with the membrane. Although the mercury rose gradually as water escaped, yet some air found its way through the bladder, and occupying the upper part of the tube, separated the liquid and bladder from each other. Under such circumstances only, air and aqueous gas could reach its lower surface. Notwithstanding this, and the gradual increase of the quantity of air, the mercurial column continued to rise, showing that the rate of the penetration of aqueous gas, is greater than that of atmospheric air, by which it could not be counterbalanced. Curious to see the effect, I tied over the summit of the tube, a bag, holding carbonic acid, which thus replaced the atmosphere. Almost immediately, the mercury gave intimation of descent, by losing its convex summit. It did fall, and carbonic acid entered through the membrane, faster than the moisture had at any time escaped.

stances dart with surprising facility, losing as their pa
rate, all cohesion, and acting repulsively. The oils are
in this regard, and camphor exhibits, because of it, (
agreeable movements, when thrown upon perfectly clean

But it is chiefly with reference to physiology, patl
practical medicine, that we see, in the foregoing experim
of much real value. They throw a particular light on tl
of respiration and cuticular absorption, and will prob
the employment of gaseous agents of cure with conf
certainty.

The experiments on the mutual action of gases and li
that although a gas may, when *alone* presented to a liqui
it has no chemical affinity, penetrate its molecular ca
will again leave it to join any gas whatever, which is l
communication with the liquid. Thus carbonic acid
oxide readily penetrates blood or water, but returns fron
the air or any other gaseous substance, which contains
acid, or nitrous oxide. It is in this way, probably, that
disappears, and an exactly equal quantity of carbonic a
it in the bronchial cells. Oxygen penetrates slowly tl
nous tissue, to infiltrate and brighten the blood; carb
immediately formed, and being a gas differing from th
of the air yet in the air-cells, its tendency is to return,
that air, and thus escapes through the trachea along wi
oxygen enters, because there is oxygen enough behin
that, and it is also an observed fact. The carbonic a
makes its escape, because invited by the molecular tiss
spheric air. Keeping up any reference to known fac
scarcely doubt the truth of our explanation, or venture t
other. The investigations of John Davy, and our caref

* The best mode of examining this property of camphor is the follc
a piece of cork, a flat four-sided prism, and attach to its narrow side
ends, and diagonally opposite to each other, two small pieces of camp
with its broad surface upon a considerable plane of *quite clean wate*
tus will regularly rotate, and that either until the camphor is con
interspace is filled with that substance, or an emanation from it.
the space, immediately suspends the motion. If a cork be grease
camphorated at the end, it will move in a direction from that end, a
derable velocity. The same thing happens when fine dry flour is
when the butt end of the cork is dipped into ether or alcohol. A
made in the upper surface of a floating cork, near the end, filled w
connected by a cotton filament with the water, it will sail about a pne
for a considerable time, always moving towards the solid end. A
being attached to the cork, and slightly inflected, the vessel may b
entirely round a circular tub.

of his experiments, with others, fully as conclusive, leave no doubt of the entire absence of carbonic acid in the blood.*

It must, therefore, be produced in one of two modes, either by the penetration of oxygen into the blood, and its union there with carbon, or the exit of carbon from the blood, to unite with oxygen in the air-cells. Now, as carbon is one of the most fixed substances in nature, and has not been proved capable of such transmission, we are, if *facts* be our guide, compelled to adopt the other theory, which is perfectly in accordance with the laws of gaseous infiltration. If it be asked how the carbonic acid is formed in the blood, at so low a temperature, we reply, that carbonic acid is actually created *at a lower temperature*, by the agency of infiltration, when oxygen gas is imbibed by a piece of fresh cold charcoal. The difference in the rate of permeation, is quited sufficient to account for the escape of all the carbonic acid formed by the infiltrated oxygen.

Our theory does not account for the production of animal heat, but it is presumed that no well-informed physiologist now seeks for it in the action of the lungs, or the process of decarbonization. The simple fact, that cold-blooded animals breathe without *any* increase of temperature, proves that mere breathing to *any amount* will not produce heat. Like all the other animal functions, that productive of heat is dependent on a normal condition of blood, and is thus *indirectly* governed by the act of respiration. As in cold-blooded animals, there is no apparatus for producing heat, respiration does not in any way influence *their* temperature. So in some of the cases quoted by John Hunter, where blue-boys maintained a temperature preternaturally great, the blood was very imperfectly decarbonized. In such cases the calorific function found some novel stimulant.

Our experiments afford ready explanations of the effect of the various gases when respired. Carbonic acid not only cuts off the necessary supply of oxygen, but also penetrates into the blood, and passes through the route of the circulation.

We perceive why nitrous oxide, so identical with oxygen in all its chemical habitudes, should act so differently on the human system. It penetrates at least sixteen times as rapidly, and probably acts then solely as oxygen would do. Hence we see why it does not

* Having filled a phial with hydrogen gas, blood was received into it from a vein, so as to exclude the agency of oxygen. When completely full of blood, the phial was closed by sheet gum elastic, and immediately subjected to the action of the air-pump. Under such circumstances, no gas of any kind could be immediately separated from the blood; but after coagulation was completed, a bubble of air, about the size of a pin's head, was perceived beneath the membrane, and that that was atmospheric air, or nitrogen, was proved by its long continuance there, without apparent diminution or escape.

exhaust us; for it not only acts upon excitability, but cre
supply of it, so that its consumption is not felt. We car
see why an animal was destroyed in ten minutes by breat
gen, while carbonic acid produced the same effect in tv
In Section I. Article IX. of his Physiological Researc
relates some curious exemplifications of the passage of ga
blood-vessels through the lungs of living animals. Fe
hydrogen gas could be set on fire, as in bubbles it esca
remotely situated *blood-vessel.* As he had used some forc
of a stop-cock and syringe adapted to the trachea, to th
retain the gas, he ascribes its entrance to that cause. W
ever that though impulsion augments the effect, yet that i
independently of any *vis a tergo*. Gases not *at all* solub
will not pass without force, but that force is, in some degi
in every act of expiration. Those soluble in blood fin
trance when not held back by the interstitial molecular p
other gases with which they enter the bronchiæ.

The emptiness of the blood-vessels after death, or
fulness of gaseous matter, is no longer a case of difficu
Always present in the air-cells after death, air and carbo
must find a ready entrance into the emptied capillaries o
always prompt to dilate through the influence of the ela
which exists in and around them in the lungs. As any
acts as a stimulant to the heart's cavities *, a gaseous ci
kept up, and the aëriform matter passes into the great
circulation.

It does not appear difficult to understand why so pene
poisonous a gas as sulphuretted hydrogen should often
intestines without injury; for, being mixed up with othe
tendency to infiltration is greatly restrained. When un
diffusion through the whole system is fearfully rapid.

'Of all the gases,' says Dr. Ure, 'sulphuretted hydr
most deleterious to animal life. A greenfinch plunged int

* In 1823, being engaged in dissecting a sturgeon, (*Acipenser br*
its heart was taken out and laid on the ground, and after a time, hav
beat, was inflated by mouth for the purpose of drying it. Hung up i
began again to move, and continued for ten hours to pulsate regularly,
and more slowly. Left at 1 A.M. in slow motion, it was found next
and hard. When last observed in motion, the auricles had become
rustle as they contracted and dilated.

With the heart of a *Testudo serpentaria*, (Snapper,) I lately
experiment, and found it beat well under the influence of oxygen, h
bonic acid, and nitrogen, successively thrown into it. Water also
perhaps more strongly, but made its substance look pale and hydropi
minute destroyed action beyond all known means of restoration.

contains only $\frac{1}{1500}$ of its volume, perishes instantly. A dog of middle size is destroyed in air that contains $\frac{1}{800}$, and a horse would fall a victim to an atmosphere containing $\frac{1}{250}$.

'Dr. Chaussier proves, that to kill an animal it is sufficient to make the sulphuretted hydrogen gas act on the surface of its body, *when it is absorbed by the inhalents.*'

One of the objections to the belief in aërial poisons most confidently urged by *antimiasmatists*, is the absence of all proof of absorption of gaseous matter, and indeed this was the sole difficulty of any real moment in the way of the triumphant establishment of the theory of miasm. Will it now be going too far to say, that this difficulty is removed, and that we can explain why miasmata affect persons so differently who reside in different apartments of the same house, or who live on opposite sides of the same street. Although being a very little nearer to the source or to the ground may not appear important, yet the difference of a few yards makes in either case a momentous distinction. Very near to its source a gaseous substance occupies a larger portion of the atmospheric space, and presents not only more matter, but matter less restrained by the molecular power of the air with which it is mingled. Not only is a greater quantity presented, but it is withheld from admission into the tissues by a slighter restraint.

As pressure unquestionably affects the rate of gaseous infiltration, *a difference in the amount of atmospheric pressure* will perhaps be considered of some importance, and *assist* in accounting for the general unhealthiness of low situation and intertropical latitudes.

Spontaneous evaporation has been long a subject of interest to the philosopher, and has not hitherto admitted of adequate explanation. Now we perceive, that in elevating moisture into the atmosphere, a very powerful agent is at work, one capable of subverting the cohesion even of solids, and of producing the continued infiltration of the atmosphere. Heat being also capable of destroying the attraction of aggregation, augments evaporation and interstitial infiltration. On this (I speak it hesitatingly) depends the power of steam. Caloric penetrates gases as they do each other, and escapes from them in exactly the same manner when substances which contain less of it invite its penetrant power in a new direction. Thus, for illustration, carbonic acid penetrates common air, and, so far as we know, will expand it, if constantly supplied, to an amount of power not yet measured. But so soon as another gas or penetrable substance is presented, it begins to withdraw from the air and to penetrate that. The hollow intestine used in one of our experiments

was powerfully inflated by its entrance, and yet as rapid
when the gas was invited outwards by the presence of
on its exterior. The resemblance of phenomena does n
Each penetrates different substances with different degree
and the *quality of the surface* is often to both as influe
character of the substance which affords it. The *fact,* th
enlargement of bulk, the *penetrativeness varying usua*
substance and surface to be acted on, being however un
tive to all gases, the *constantly diminishing rate of prog*
issuing out again when invited by new substances, or a
when mechanical compression is applied,—all afford evid
logy as perfect as is perhaps ever offered to the view of p

We are struck with its resemblance *to water* in c
Highly concentrated caloric invites the penetration of
and perhaps of all solids; and thus, while held in sol
they obtain a penetrativeness themselves which does n
belong to them, and are elevated into the atmosphere
specific gravity, however high, or of atomic weight, how
derable. Some facts, not yet sufficiently studied, lead
perhaps hasty conjecture, that even the *decomposing* i
caloric is owing to this power. Water exercises it in t
some cases, such as that of acetate of lead.

The great length to which my remarks have unexp
tended, and the call of the printer, prevent me from goin
the consideration of the connexion of our experiments w
logy and therapeutics. Their bearing on these depa
medical science will furnish subject matter for a future
the mean time, we feel entitled to believe that we better c
some of the phenomena of colic, tympanitis, and emphy
see more clearly the cause of the value of certain method

Bichat was among the first to produce the passage of air
kinds into the blood-vessels and cellular tissue of the lungs,
it into the air-cells and there confining it. Even when
vessels were full of froth, and emphysema became ext
could perceive not the slightest laceration of the bronchi
the impulsion was moderate, the air passed only into t
vessels; when more violent, its presence became manif
cellular tissue. In certain cases referred to by authors, v
ertion, laborious respiration, and severe flatulency of the
have forced air into the blood-vessels and cellular tissue.
produced also tympanitis, and few practised physicians are
of the fact, that great gaseous distention of the abdomen

peared without the *apparent* escape of any wind. When we consider attentively the laws by which are regulated the entrance and exit of gases under the action of their penetrativeness, we feel scarcely at a loss to understand these phenomena.

The prodigious accumulation of gas in the stomach and bowels, in hysteria and epilepsy, may be explained, by supposing the air, which exists by infiltration in every part of the animal economy, to be forced by the violent compression of spasmodic action into the hollow viscera, where already existent gases invite its entrance. In some experiments on the effect of certain gases on living cavities, made by my ingenious friend Dr. Finley, their escape was so rapid as to create surprise *.

The establishment of the fact of the penetration of liquids, each according to its peculiar rate, and the modifications of that rate dependent on extrinsic force, such as impulsion or invitation, electricity, &c. teach us many valuable lessons both in philosophy and medicine. Especially I would invite attention to the cause of the remedial influence of *pressure*, as auxiliary to other means of cure.

Recapitulation.—1st. Substances formed of organic matter are generally penetrable by gases of all kinds, and by several, if not by all liquids.

2d. Each animal or vegetable tissue is differently penetrable as to time by different fluids.

3d. But all fluids penetrate any particular substance at rates susceptible of being ascertained. The gases retain the relation observed by reference to one substance in all other cases. Whatever may be the greater or less penetrability of any given tissue, the gases penetrate it, relatively to each other, according to the ratio observed in experiments on other tissues.

4th. The *ratio* is not so uniform in the instance of denser fluids. Liquids, though rateable with regard to permeation of any given substance, do not act similarly on different organic substances. Thus water penetrates most, if not all animal tissues, better than any other liquid whatever, and consequently passes through them to accumulate in *any of its own solutions*, and in alcohol or ether; while these two latter substances penetrate gum elastic with more facility than either water or its solutions. Therefore, with regard to gases, the *ratio* of penetration depends on them alone; while, in the case of liquids, it depends on the joint agency of both liquids and tissues.

5th. When the quantity of the fluids is limited, there is a gradu-

* North Amer. Med. and Surg. Journ. No. VI. 1827.

ally diminished rate of progression as the infiltration pro
proportional to the state of dilution, and ceases when the
have become, on both sides of the membrane, of uniforn
unless some extrinsic power is then operative.

6th. The power of the penetrativeness is very conside
certainly superior to that of two, and *possibly* equal to
that of forty atmospheres.

7th. Penetrativeness acts not only on organic tissues,
gases and liquids, and with apparently equal power on
after permeating a membrane, the gas or liquid goes
molecular tissue of the gas or liquid beyond, and no pres
the membrane can bear acts as a restraint on the progre

8th. Although of such high mechanical power, the pen
be, to a certain degree, affected by extrinsic agency. Th
or attraction will cause permeation, where it would no
take place, as when a single gas or liquid travels not on
but beyond a membrane, where there exists nothing to
which it would not do, unless subjected to propulsion.
possessed of hydragogue powers, acts on water in a simil
causing it to collect on either side of an animal membra
sure, although no other liquid is there to receive it.

9th. The penetrativeness of gases for each other seem
velocity, but not in force.

10th. Reference to the abovementioned laws and
agencies enables us to explain many pheuomena hitherto
understood. We, by means of them, comprehend the u
stitution of mixed gases in any vessel, or in the atmos
withstanding the greatest difference in specific gravity.
the diffusion of odours, the nature and power of spontane
ration, and the probable nature and progression of cal
slow conduction. It affords us new views of the theory
tion, and accounts, in that process, for some well-ascerta
for which there previously existed no adequate explanatio

It shows us how emphysema and tympanitis may happ
secretion of gases, or lesion of tissue, and how a sponta
may be produced. It leads to the probability of the e
gaseous matter of very various kinds in almost every p
animal frame, resident there molecularly, and not *en*
susceptible of being collected into mass in the great cavi
cells of the tissue, or the blood-vessels, by mechanical o
influence, or the attractive interstitial agency of other ma

It teaches the important truth, that water is the gre

infiltrator and diluent, a knowledge of whose habitudes will be thus rendered both clearer and more useful.

Before closing my remarks, I am happy to be enabled to say, that a considerable number of my medical friends visited my laboratory, and saw for themselves the verifications of my statements. I solicited their observation, both for the confirmation of my own impressions, and for the greater readiness of reception which the public always affords to facts which have appeared in a similar light to several different individuals of adequate judgment.

In my next I hope to present a table of the rates of penetrativeness of liquids for animal membranes. I hope also to ascertain the amount of force. On the relation of the respirable gases to the blood, and other liquids, I possess already many interesting facts, which will be then promulged.

Philadelphia, September 15th, 1830.

Since the foregoing paper was sent to the editor of this Journal, I have had an opportunity of reading M. Dutrochet's short essay, entitled '*Nouvelles Recherches,* &c.' In it I find that the author has discovered his mistake relative to the action of acids in general, but has fallen into one quite as important, respecting the agency of diluted sulphuric acid. He *now* considers it a *nullifier* of endosmose, instead of a promoter of exosmose, being not only itself inactive, but the cause of inactivity in other solutions. Feeling confident of the power of diluted sulphuric acid to receive as much water as the animal membrane could convey, I, in conjunction with Professor Finley, carefully repeated our experiments on that substance. In every case, where the solution exceeded 1°, (Beaumé,) it was adequate to the occupation of as much water as could be presented by the membrane. At 2°, 11°, and 25°, the acidulous liquid gave the same *rate* of aqueous infiltration as did alcohol, ether, &c. A solution of sulphate of soda, at 11°, and at 3° Beaumé, and a solution of ammonia at 40° centesimal alcometre, being infiltrable by water, at a rate not less than that of the animal membrane, of course afforded, when compared with that liquid, exactly the same results. Although all these substances gave evidence of having been contemporaneously transmitted through the membrane, yet the quantity, easily appreciated chemically, was not so great as to make a sensible difference in the altitude of the column, whose rise represented the transmission of water. When, by the entrance of a considerable quantity of water, the acid was so far diluted as to intermingle with it more slowly than the membrane could present it, a rapid diminu-

tion of ascent ensued. At length, so little was received, a
compensate for the effect of gravitation. Finally, the
power of reception being below the effect of gravitation
descended again, and the two columns reached a com
Seeing these causes of change, we can estimate the rate
observing the time taken to traverse a *short space*, and i
diately at the commencement of the experiment. Unle
penetrant liquid be of much more power of reception than
necessary, its dilution soon destroys its adequacy, and l
apparent rate, just as, in forming solutions, we percei
diminution of solvent power as the point of saturation is a
In addition, when both liquids are traversing the membr
same time, there is a progressive approach to a commo
vourable to repose. M. Dutrochet, therefore, by observing
of solutions of different strength, in a considerable lengt
(an hour and a half,) obtained results, not the act of the r
but of the solution—not the maximum effect of the tiss
constantly diminishing action on water of a gradually dil
tion. His results might therefore have been anticipated
lation; for, as water dissolves less and less, in a given tin
soluble substance, so a soluble substance acts on water
to it in a steadily declining ratio. When the demand fo
above the powers of supply through the membrane, the ra
regulated solely by the water and membrane, and is the
a great variety of substances. When the demand becomes
the supply, the case is one of simple solution, with which
brane may be supposed to have no connexion. It is th
the part of a still surface of water.

The following facts, ascertained at an early period of thi
gation, will place this principle in a strong light. An inv
phon, such as already described, was filled with atmosphe
portion of which, by placing thirty-four inches of merc
long limb, was confined in the shorter one. There being
same gas on both sides of the membrane, the current set i
rection given by impulsion, and the long column fell—

$\frac{3}{8}$ths of an inch	in 2 hours and 30 minutes, or 50 min.
$\frac{3}{8}$ths more	in 2 hours and 39 minutes, or 53
$\frac{3}{8}$ths more	in 2 hours and 26 minutes, or 48$\frac{2}{3}$rds
$\frac{1}{8}$th	in 1 hour and 1 minute, or 61

$1\frac{1}{4}$ inch in the whole in 8 hours and 36 minutes.

At this period of the experiment, when the mercurial column stood two inches and a half lower, *proportionally*, than at the commencement, a vessel containing carbonic acid gas was placed over the shorter limb. Immediately the long column began to rise—

$\frac{2}{8}$ths of an inch	in 20 minutes,	or 10 minutes	per $\frac{1}{8}$th.
$\frac{1}{8}$th more	in 10	or 10	per $\frac{1}{8}$th.
$\frac{1}{8}$th	in $12\frac{1}{2}$	or $12\frac{1}{2}$	per $\frac{1}{8}$th.
$\frac{1}{8}$th	in $37\frac{1}{2}$	or $37\frac{1}{2}$	per $\frac{1}{8}$th.
$\frac{1}{16}$th	in 60	or 120	per $\frac{1}{8}$th.

The column appearing stationary, was left nine hours unobserved; at the end of that time—

$\frac{13}{8}$ were lost	in 9 hours,	or $41\frac{1}{2}$ min. per $\frac{1}{8}$th.
$\frac{5}{8}$ths	in 3 hours 21 minutes,	or $40\frac{2}{10}$ min. per $\frac{1}{8}$th.
$\frac{2}{8}$ths	in 1 hour 24 minutes,	or 42 min. per $\frac{8}{8}$th.

At this moment the mercury came into contact with the membrane, all the gas being excluded.

The uniformity of descent, and the progressively diminishing rise, are striking facts. It will also be observed, that the carbonic acid *seemed* to cease action, because of a weight of nearly thirty inches of mercury; whereas, in another experiment, sixty-three inches were readily driven upwards. We therefore easily perceived the cause of Dutrochet's mistake.

One other nullifier of endosmose is thought by Dutrochet to exist. A solution of hydro-sulphuret of ammonia at first quickened, and then totally arrested the motion of the fluid in the stem of his endosmometer; for which he accounts by supposing the final production of sulphuretted hydrogen in the solution, and the extinctive agency of *that.*

The great activity of gaseous sulphuretted hydrogen, on which Dutrochet made no experiments, led me to suspect that its solution was gifted with considerable penetrant power, and by thus counterbalancing the amount of penetrating water, appeared to act in arrest of motion, presenting just such a case as we witnessed when comparing together olefiant gas and arsenuretted hydrogen. For verification, a solution of sulphuretted hydrogen in water was, by means of the inverted syphon, compared with water, and scarcely any motion observed. A similar solution, enclosed by an animal membrane in a wide-mouthed bottle, was placed in a vessel of pure water, mouth downwards. In this instance the membrane gave no sign of inflection at first, but after several hours showed a slight bend inwardly. In both these cases the portion of liquid, originally clean water, when tested by acetate of lead, afforded the deep

black precipitate, indicative of the presence, abundar phuretted hydrogen.

In a second experiment, with a solution of sulphurette enclosed in a bottle, the water placed in the outer vess the slightest trace of acetate of lead. Scarcely was th mersed before the precipitation of the lead commenced. solution of sulphuretted hydrogen in water was, by meai verted syphon, compared with alcohol confined in its s In this instance, and in every repetition, the movement fested towards the alcohol, the rise of which showed th: trative power of liquid sulphuretted hydrogen is somew than that of water, and of course much greater than that These experiments were made with extraordinary care, them seemed to hang the fate of this whole question c The whole doctrine of regular rate of penetration, &c. i the ground if my trials had been confirmatory of the obs M. Dutrochet.

The totally different results, as to the force of pen which M. Dutrochet and myself have arrived, render few words of explanation.

It will be conceded that the fairest mode of estimating when the liquid is fresh and the process just well begun. tude of the highest column of mercury which it can raise sent its power, and that column should, if possible, be la once. In this manner I proceeded, and found that b and gum elastic were broken by a column higher than inches, although, just before giving way, the column was could rise solely by the power of penetration, no other kr of motion being present. But M. Dutrochet, laying or less than sufficient, left his apparatus to raise that colum or two, until the process of elevation ceased. The h reached he considered as representing the power of *endos* attentive reader will readily perceive, in this plausible e the same error which deprived the facts, as to time, of va solution had become diluted, and the water on the othe become impregnated, and, independently in a great mea weight of the column, the causes of production of peneti rents had ceased, and these beautiful experiments reporte weight which could be raised, but the time required by su tion to distribute its qualities uniformly, or nearly so, on of the membrane Left in that state the column desc evincing the cessation of penetration, not its *forcible* r

This is well proved by his latest experiment, in which having raised a column of mercury by the penetration of water into a solution of gum Arabic to twenty-eight inches, and while still rising, he replaced the external water by a solution of gum Arabic, when an immediate ascent was observed. The substitution of clean water again caused an elevation of the column.

On the whole, captivating as is the method, and elegant as are the experiments, of this little volume of M. Dutrochet, it does not bring additional support to his doctrine of *endosmosis*. Yet whatever may be the issue of the experimental investigation to whose rigid scrutiny this most important subject is committed, the philosopher and physician can scarcely find language adequately to express the obligation, the high obligation, under which science has been laid by the elegant labours of M. H. Dutrochet. In him we discover the *punctum saliens* of a principle which is the master spirit of animal and vegetable motion, the ruling power of chemical science, the governing influence of atmospheric composition, the presiding genius of respiration, circulation, and nutrition, the cause of disease, and the restorer of health. But whatever may be *now* his fame, how little is it compared to that which may be anticipated for him by one who takes even a careless view of the mighty field of novel observation just redeemed from the rich wilderness of nature! This tribute is paid the more unhesitatingly because it is due, and because I have so freely criticised and censured where the cause of science and truth demanded severity. It is in great men, and in great discoveries, that blemishes are most ungraceful and most injurious. The very magnitude and extent of the principle for whose detection we must thank Dutrochet, give a fearful importance to the slightest coextensive errors.

September 18th, 1830.

ANALYSIS OF BOOKS, &c.

Memoir of the Life of Thomas Young, M.D., F.R.S., Associate of the Royal Institute of France, &c., &c., with logue of his Works and Essays. London, 1831. 8vo.

THIS tribute to the memory of one of the most gifted m distinguished philosophers of the age, has been printed for private distribution. It has almost the interest of an a graphy, having been drawn up by a gentleman who had the tage of a long and intimate acquaintance with him, from som memoranda of Dr. Young's own writing, in the possession o connexion. The author modestly states, that 'having nev engaged in the pursuits of accurate science, he feels himself petent to give more than an imperfect sketch, which he trust filled up hereafter by an abler hand.'

No apology, it is presumed, will be necessary for transfe these pages the substance of this account of Dr. Young, wh his connexion with the Royal Institution, as one of its pro and as the editor of the first series of its Journal, independen claims as a scholar and philosopher of the first class, es merits distinguished notice in this work.

Thomas Young was born at Milverton, in Somersetshire, 13th of June, 1773. His parents were both of them Quak of the strictest of that sect; his mother was a niece of Dr. Brocklesby, a physician of eminence, well known from l nexion with the distinguished literary and political character time, and who numbered among his most intimate friends, J Burke, and Windham.

To the influence of the early impressions of the Quaker te Young 'was accustomed to attribute, in some degree, the p so eminently possessed of an imperturbable resolution to e object on which he was engaged, which he brought to bear o thing he undertook, and by which he was enabled to work own education almost from infancy, with little comparative as or direction from others.' The earliest years of Dr. You chiefly passed in the family of his maternal grandfather, Mr Davis, of Minehead, who, in the midst of mercantile avocati cultivated a taste for classical literature, with which, by ear deavour, he seems to have imbued the mind of his grands appears to have been a forward if not a precocious child. I that he could read with fluency when he was two years old; a after this, in the intervals of his attendance on a village scl tress, he committed to memory a number of English verses, a was taught to recite some Latin poems, the words of w

retained without difficulty, although unacquainted with their meaning. Before he was six years old, he was sent to a school kept by a dissenting minister at Bristol, where he remained about a year and a half, and became essentially his own instructor, and had generally studied the last pages of the books used before he had reached the middle under the eye of his inefficient master.

It has been remarked, 'that the early quickness with which learning is imbibed, is not always the indication of permanent ability; facility of acquiring does not in general establish a power of retention; whilst what is received with difficulty, is frequently preserved and digested in the mind.' The case of Dr. Young, however, was one of those happy exceptions to this remark; and in none of those extraordinary instances recorded by Baillet in his work 'sur les Enfans célèbres par leurs Etudes,' is there a more remarkable instance of the promise of youth being realized in the man.

To one of those accidental circumstances, which, though they do not create a peculiar genius, yet very often determine its bent, may be attributed that love of science which distinguished Dr. Young, and which (says his biographer) 'had probably no small influence on the issues of his future life.'—'His father had a neighbour, a man of great ingenuity, by profession a land-surveyor; and in his office, during his holidays, he was indulged with the use of mathematical and philosophical instruments, and the perusal of three volumes of a Dictionary of Arts and Science. These were to him sources of instruction and delight of which he seemed never to be weary.'

In his visits to this neighbour, Young had acquired some knowledge of the art of land-surveying, and used to amuse himself in his walks by measuring heights with a quadrant. In 1782 he was placed at the school of Mr. Thompson, at Compton, in Dorsetshire, where he went through the ordinary course of Greek and Latin, with the elements of mathematics; here also he had access to a moderate miscellaneous library, and by rising earlier and sitting up later than his companions, with the assistance of a schoolfellow, he acquired some knowledge of the French and Italian languages.

Botany having about this time engaged his attention, and desiring to possess a microscope for the purpose of examining plants, he attempted the construction of one from the descriptions of Benjamin Martin. This led him to optics; and having procured a lathe in order to make his microscope, like most young experimenters, he forgot or neglected science, for a time applied himself to the acquirement of manual dexterity, and every thing gave way to a passion for turning, until, falling upon a demonstration in Martin's Philosophy, which exhibited some fluxional symbols, he was not satisfied until he had read and mastered a short introduction to the doctrine of fluxions.

Before he quitted school, a Hebrew Bible being left in his way, he began by enabling himself to read a few chapters; this led him to the study of the other principal oriental languages; and on quit-

ting Mr. Thompson's, at the age of fourteen, it appears that l
more or less versed in Greek, Latin, French, Italian, Hebrew, P
and Arabic; and had laid the foundation of that calligraph
for which he was afterwards so remarkable, and in which he ı
even the neatness and beauty of the pen of Porson.

He was about this time attacked by symptoms of what his
feared to be incipient consumption, but by the attention of hi
Dr. Brocklesby, and Baron Dimsdale, his health was restor
indisposition scarcely interrupted his studious labours, and it
that 'he merely relieved his attention by what to him stood
place of repose—a course of Greek reading in such auth
amused the weariness of his confinement.'

In the year 1787, he met, at the house of a relation, a fi
Mr. David Barclay, of Youngsbury, in Hertfordshire, who w
wishing to form an arrangement for the education of his gr
and it was at length agreed that the youths should pursue th
dies together, under a private tutor in Mr. Barclay's house
tutor, however, did not come, and Young, who was only a y
a half older than his companion, took upon himself provision
office of preceptor. They were afterwards joined by Mr. H
author of the 'Calligraphiæ Græca,' who was of somewhat
years, and then seeking to perfect himself in the higher bra
classical attainments. But Young did not relinquish the task
undertaken, and continued to be the principal director of the
of the whole party.

Thus passed the five years from 1787 to 1792, the summe
spent in Hertfordshire, the winters in London, and with r
assistance than that of a few occasional masters, when in Lo
had rendered himself singularly familiar with the great w
antiquity, keeping ample notes of his daily studies. 'His
was not,' says his biographer, 'for the purpose of merely
words and phrases, and the minuter distinctions of dialects,
invariably also directed to what was the end and object of tl
he laboured through;' he had drawn up an admirable an
the various conflicting opinions of the ancient philosophers,
probable that the train of thought into which this led l
not without its effect in mitigating his attachment to the
views of the Quakers. He had now acquired great facility in
Latin; composed Greek verses, which were well received by
tinguished scholars of the day, and applied himself assiduous
higher mathematics. To the studies of botany, zoology, a
cially of entomology, he at the same time paid considerable a

In the winters of 1790 and 1791, he attended the chem
tures of Dr. Higgins, and having previously prepared hi
reading on the subject, he began to make simple experimen
own. But he is said to have been at no period of his life
repeating experiments, or even of originating new ones; 'c
ing that, however necessary to the advancement of scien

demanded a great sacrifice of time, and that when the fact was once established, that time was better employed in considering the purposes to which it might be applied, or the principles which it might tend to elucidate.' At Dr. Brocklesby's recommendation, and under his superintendence, he now directed his views to the studies necessary for the practice of physic, and made to him a regular report of his literary and scientific pursuits. The Doctor lived in intimacy with Mr. Burke and Mr. Windham, and having communicated to them some of his nephew's Greek translations, he was introduced to those two distinguished persons. Mr. Burke is said to have been so greatly struck with the reach of Young's talents, and the extent of his acquirements, and more particularly by his great and accurate knowledge of Greek, that he was in no small degree indebted to the good offices of that eminent statesman for the interest which his uncle afterwards took in his future settlement in life.

'It may probably be considered that it was at this period his character received its development. He was never known to relax in any object which he had once undertaken. During the whole term of these five years, he never was seen by any one, on any occasion, to be ruffled in temper. Whatever he determined on he did. He had little faith in any peculiar aptitude being implanted by nature for any given pursuits. His favourite maxim was, that whatever one man had done, another might do; that the original difference between human intellects was much less than it was generally supposed to be; that strenuous and persevering attention would accomplish almost anything; and at this season, in the confidence of youth and consciousness of his own powers, he considered nothing which had been compassed by others beyond his reach to achieve, nor was there anything which he thought worthy to be attempted which he was not resolved to master.'

His biographer thinks, with justice, that 'this self-conducted education in privacy was not without its disadvantages—that though the acquirements he was making were great, he was not gaining that which is acquired insensibly in the conflict of equals in the commerce of the world—the facility of communicating knowledge in the form that shall be most immediately comprehended by others, and the tact in putting it forth that shall render its value immediately appreciated.'

His first communications to the press were made in 1791, through the medium of the 'Monthly Review,' and the 'Gentleman's Magazine;' and towards the end of 1792 he established himself in lodgings in Westminster, where he resided two years, attending the lectures of Baillie and Cruickshank on anatomy, and was, during that period, a diligent pupil of St. Bartholomew's Hospital.

In 1793, he made a tour in the west of England, principally to study the mineralogy of Cornwall; and about this time the Duke of Richmond, then Master-General of the Ordnance, who had long been a friend of his uncle, offered him the situation of assistant-secretary in his house. Mr. Burke and Mr. Windham recommended him to proceed to Cambridge and study the law, but his own predilec-

tions and habits decided him, upon due consideration, t
in favour of the practice of physic, as most congenial to
pursuits, and to which the position occupied by his uncl
offer a favourable introduction.

In this year he communicated to the Royal Society]
tions on Vision, and his Theory of the Muscularity of th
Lens of the Eye, which became the object of much
John Hunter laying claim to having previously made th
Dr. Young was soon after elected a fellow of the Ro
when he had just completed his twenty-first year; and ir
of 1794 he went to Edinburgh, and attended the lectures
Black, Munro, and Gregory.

He now separated himself from the Society of Qu
amidst the most active pursuit of his medical, scientific, a
labours, still found leisure for cultivating those arts in
early education had left him deficient. The versatility o
reminds us of what has been recorded of the Admirabl
It is said that 'everything, be its nature what it mig
him a science; and that whatever he followed, he follow
cally. Of music he was extremely fond, and of the scien
he rendered himself a master. He had at all times gre
activity, and in youth he delighted in displaying it.'—'H
his graver studies by cultivating skill in bodily exer
lessons in horsemanship, in which he always had great ple
practised, under various masters, all sorts of feats of pers
in which he excelled to an extraordinary degree.' As a cl
anecdote, it is recorded that, in instructing himself in a
made it the subject of a diagram.

Toward the close of 1795 he went to the University of
where he took his doctor's degree, and excited the won
laborious school by his extraordinary attainments and al
dible industry. Here he composed a treatise 'De Corpo
Viribus Conservatricibus,' leaving few volumes unconsu
had any connexion with the subject he was treating. H
posed visiting Italy previously to his return to Englan
prevented by the victories of the French; he therefore pı
Dresden, for the purpose of studying the works of Italia
galleries there, and of comparing what he saw with tha
had learnt of them from the lectures of the professors at
He also made a short visit to Berlin.

'During his residence in Germany he gained a very genera
rate acquaintance with its language and literature, which l
throughout his life; he remarked that he found in German
new inventions, singularly, and somewhat pedantically, com
the habit of systematizing old ones, and of giving an importanc
in themselves trifling, which in his case rather confirmed
habit of dwelling on minutiæ more than his subsequent exp
him to think was advantageous.'

On his return to England he entered himself of Emmanuel College, Cambridge, of which Dr. Farmer, an intimate friend of his uncle, was then master. He proceeded to take his regular degrees in physic in that university, but did not attend any of the public lectures, contenting himself with pursuing the various studies in which he was engaged, living on terms of intimacy with the most highly-gifted members, and discussing subjects of science with the professors, but finding no rival in the variety of his knowledge, and few competitors in most of its branches.

Dr. Brocklesby died in 1797: part of his fortune, his books, his pictures, and his house, he left to Dr. Young, who now found himself in circumstances of independence, surrounded by a circle of distinguished and highly valuable friends, which he continued to prize and to enjoy through life. When his residence at college was completed he settled himself as a physician in London, in Welbeck Street, where he continued to reside during twenty-five years.

In 1801, Dr. Young was appointed Professor of Natural Philosophy in the Royal Institution, where he continued for two years to lecture alternately with Sir H. Davy. In 1802, he published his 'Syllabus, a Course of Lectures on Natural and Experimental Philosophy, with Mathematical Demonstrations of the most important Theorems in Mechanics and Optics.' This syllabus contained the first publication of his discovery of the general law of the Interference of Light, being the application of a principle which has since been universally appreciated as one of the greatest discoveries since the time of Newton, and which has subsequently changed the whole face of optical science *. As a lecturer at the Royal Institution Dr. Young was not eminently successful, for though his lectures were replete with interesting original matter, he was not happy in conveying it in a sufficiently intelligible manner to the capacities of a mixed audience, consisting in a great degree of persons of fashion and of the world. Dr. Young's style and manner were quite opposite to those of his eminent colleague, Davy; he was compressed and laconic, and presumed his audience better instructed in the arcana of science than such an assembly could possibly be: it has even been said that it would hardly have been possible for men of science to have followed him at the moment without considerable difficulty.

At this period Dr. Young became, jointly with Davy, editor of the Journal of the Royal Institution; the first volume, and part of the second, were published under his superintendence. It was also at this time that he gave his two Bakerian Lectures on the subject of Light and Colours, to the Royal Society. Developing the law of interference, and entering into all the details of the theory to which it leads; dwelling upon the difficult points, at the same time, with

* It was not until the year 1827, that the importance of this law could be said to be fully admitted in England: it was in that year that the Council of the Royal Society adjudged Count Rumford's medal to M. Fresnel, for having applied it, with some modifications, to the intricate phenomena of polarized light.

more candour than might have been consistent with his
he been anxious to obtain proselytes.

'In the summer of 1802, he accompanied the present D
mond, and his brother Lord G. Lennox, in his medical
Rouen, and in an excursion from thence to Paris, was firs
the sittings of the National Institute, at that time attended b
where he made the acquaintance of several leading members
tinguished body, into which he himself was eventually elec
return he was constituted Foreign Secretary to the Roya
office which he held during life, being long their senior officer
one of the leading and most efficient members of their counci

In 1804, he married Eliza, daughter of J. P. Maxw
Cavendish Square,—an union to him productive of uninte
piness during the remainder of his life. At this time
his professorship in the Royal Institution, from an er
pression that it would be likely to interfere with his s
medical practitioner. The remarks of his biographer on t
must not be withheld.

'His resolution at that juncture was to confine himself
part to medical pursuits, and to make himself known to the
other character. But he had resolved on that which to
possible. He never slackened either in his literary or
researches. He was always aiding, and always willing to b
sellor of any one engaged in similar investigations. He was
first circles of London, amongst all who were the most en
nature of his habitual avocations was necessarily well k
therefore, in putting forth his non-medical papers separatel
mously, he was making a fruitless as well as voluntary sac
general celebrity to which he was entitled; and shrinking, as
the cumulative reputation which he must otherwise have
waived, in some degree, the advantage which is given by a
towards the pursuit of even professional success.'

In 1807, Dr. Young published his 'Course of Lectures
Philosophy and the Mechanic Arts,' in two volumes, 4
of first-rate merit, which cost him nearly five years' labou
The mass of references contained in the second volum
works which the student engaged in minute inquiries in
may consult with advantage, affords evidence of the extens
and industry of this eminent philosopher. Owing to th
the booksellers engaged in the publication of Dr. Young'
the immediate sale of the work was so greatly injured tha
repay the expenses of the publication. Indeed, for son
great merits were not so extensively appreciated in Eng
the Continent; but at length justice has been done t
country which gave it birth,—it is a mine to which every o
in scientific pursuits must have recourse with advantage,
less true that 'it contains the original hints of more t
claimed as discoveries, than can perhaps be found in a
duction of any known author.' 'One of the men most di
for science in Europe has been known to say, that if his l

on fire, and he could save only one book from the conflagration, it should be the Lectures of Dr. Young.'

In 1810, Dr. Young was appointed Physician to St. George's Hospital; but his private practice, though respectable, was never extensive. His biographer has, we think, pointed out the true cause with great discrimination: 'In his profession, his published labours would prove him to have been of the most learned of scientific physicians, and his judgment and acuteness were equally great; but in the practice of medicine he was not one of those who were likely to win the most extended occupation among the multitude. He was averse to some of the ordinary methods by which it is acquired. He never affected an assurance which he did not feel, and had, perhaps, rather a tendency to fear the injurious effects which might eventually result from the application of powerful remedies, than to any overweening confidence in their immediate efficacy. His treatises bear the same impress. That on Consumption is a most striking instance of his assiduity in collecting all recorded facts; and his abstinence from drawing inferences from isolated cases, or putting forth that which he did not feel was established with certainty. Possibly he herein was an example that increase of knowledge does not tend to increase of confidence, and that those whose acquirements are the greatest meet in the progress of their investigations with most that leads to distrust.'

Dr. Young had previously given a course of lectures on the Elements of the Medical Sciences at the Middlesex Hospital, of which a syllabus was published in 1809. These lectures, he himself said, were little frequented, 'on account of the usual miscalculation of the lecturer, who gave his audience more information in a given time than it was in their power to follow.'

In 1813, he published his 'Introduction to Medical Literature, including a System of Practical Nosology;' a work of considerable labour and of the highest practical utility. To this work he prefixed a preliminary 'Essay on the Study of Physic,' partly founded on that of the German Professor Vogel, in which is contained his own conception of the qualities requisite to constitute a well-educated physician, by which it will appear that his notion of the character was elevated above the ordinary standard of humanity: 'he enumerates nearly every possible quality of which man could wish, but of which few could hope, the attainment.'

Dr. Young was a frequent contributor to the Quarterly Review, having been induced, at the instance of his friend Mr. George Ellis, to furnish articles on medical subjects. His communications, however, soon branched into other lines, connected with the higher departments of science, and containing frequently more of original research than of immediate criticism. In the catalogue of his writings, which accompanies this memoir, will be found a list of his papers in that journal. We shall only here mention an admirable philological dissertation on the Structure of Language, contained in the

review of Adelung's Mithridates, Vol. X., October, 181
remarkable, as it was the immediate means of leading
investigation of the lost literature of Ancient Egypt.
of his discoveries on this subject is given in the w
biographer, because an unjust attempt has been made to
Dr. Young the merit of having first discovered a key
glyphics.

'In the year 1814, Sir William Rouse Boughton had brou
from Egypt some fragments of papyri, which he put into
Dr. Young; the fragment of the Rosetta Stone having ab
been deposited in the British Museum, and a correct cop
inscriptions having been engraved and circulated by the So
quaries. Dr. Young first proceeded to examine the Encho
tion, and afterwards the sacred characters; and, after a m
rison of these documents, he was enabled to attach some "
Egyptian Papyri, and on the Inscription of Rosetta," *c*
interpretation of the principal parts of both the Egyptian
on the pillar, to a paper of Sir William Boughton's which v
by the Society of Antiquaries in 1815, in the 18th volume
chæologia.

'Dr. Young now found that he had discovered a key to
rature of Ancient Egypt. He had occupied himself, though
riving from it the assistance he at first expected, in the
Coptic and Thebaic versions of the Scriptures; but having
self of the nature and origin of the Enchorial character, he
result to the world anonymously in the Museum Criticum c
part vi., published in 1815; being then determined to pros
covery, but at the same time abstaining from claiming it in
stantive form, from the resolution he had previously taken
only as a medical author.

'The labour he bestowed on these investigations, and th
and accuracy with which he copied the papyri and c
materials which came into his hands, would be nearly incre
who had not access to him whilst employed on this pursuit.

'In 1816, he printed and circulated two additional lette
his hieroglyphical discoveries and the inscription of Rose
addressed to the Archduke John of Austria, who had rec
this country, the other to M. Akerblad. These letters anno
gress of the discovery of the relation between the Egyptia
and hieroglyphics, forming the basis on which Dr. Young
inquiries, as well as of the system afterwards carried further
by M. Champollion, whose attention had long been direct
studies, and in which he has since so greatly distinguished h
letters were *first published* when reprinted in the seventh n
Museum Criticum, in 1821; and were, with the former le
work, beyond all question or dispute, the earliest announce
discovery of a key to a character which had remained unin
ages.'

The whole results of his discoveries on this subjec
brought out in a perfect and concentrated form in the art
published in the Supplement to the Encyclopædia Br
which work Dr. Young furnished sixty-three articles, sc

graphical, and literary, which are designated by two consecutive letters of the sentence *Fortunam ex aliis.* His adoption of this motto is deemed 'to have been caused by the consideration that he had not then succeeded to his wish or expectation in his profession, and that he had reason to complain that the extent and utility of his labours in science, after having been fully appreciated by the philosophers on the Continent, had not appeared to have met with the same acceptance among his own countrymen.'

This feeling was, however, transitory, and was, indeed, hardly well founded. The fact is, that Dr. Young, by those best qualified to form a judgment, was acknowledged to rank in the highest scale, if not to stand at the head, of the men of science and literature of England, and his reputation was duly appreciated on the Continent; but the studious concealment with which his manifold contributions to the stock of human knowledge in science and philology were stolen into the world, prevented him from enjoying that wide-extended fame with his countrymen to which he was justly entitled, and which he really did enjoy in an extensive circle of truly eminent friends.

The philosophical articles of Dr. Young in the Supplement to the Encyclopædia Britannica, contain the results of his most elaborate investigations. His biographical sketches in the same work are admirably given; and the Life of Porson, in particular, has been pointed out as 'a masterly production, containing a very interesting indication of some of Dr. Young's opinions both on the value of classical studies and on the mechanism of the human mind.' The article LANGUAGES, in the same work, contains the fruits of his investigations on the subject, into which he had been led when engaged in reviewing Adelung's Mithridates for the Quarterly Review.

Early in 1817, Dr. Young paid a second visit to Paris, and was received with that consideration due to him in the scientific circles there. He was happy in renewing his intercourse with Humboldt, Arago, Cuvier, and Gay-Lussac; and such was the pleasure derived from his flattering reception, that, having occasion to return to London for a short period, he was induced to make a second visit of a few weeks to Paris in the summer of the same year.

In 1818, he was appointed one of the Commissioners for taking into Consideration the State of the Weights and Measures employed throughout Great Britain. To this Commission he acted as Secretary, and furnished the scientific calculations and the account of the measures customarily in use, attached to the three Reports laid by them before Parliament. It appears to have been Dr. Young's opinion that, 'though theoretically it might be desirable that all weights and measures should be reducible to a common standard of scientific accuracy, yet that practically the least possible disturbance of that to which people had been long habituated was the point to be looked to, and on this ground he was extremely averse to unnecessary changes.'

Towards the end of the same year, Dr. Young was appointed

Secretary to the Board of Longitude, with the charge o
vision of the Nautical Almanack, having been before na
the Commissioners without his previous knowledge. T
ment was to him a very desirable one, though the labour
involved him was great. His anxiety to increase his m
tice henceforth ceased, and it made that the business of h
had always been congenial to his inclination.

For the first sixteen years after his marriage, Dr. You
accustomed to pass his summers at Worthing, with a
practice of his profession. He now discontinued his vis
voted the summer of 1819 to a hasty tour into Italy. In
months he visited the most remarkable places, and ex
Egyptian monuments preserved in the museums of that
turning to England by Switzerland and the Rhine.

From the year 1820 to the close of his life, Dr. Youn
to furnish a variety of astronomical and nautical collect
Brande's 'Journal of Science,' together with some
papers.

'In 1821 he published anonymously an "Elementary Il
the Celestial Mechanics of La Place, with some addition
the motion of Waves and of Sound, and to the Cohesion
This volume, and the article "Tides" in the Supplement to
pædia, Dr. Young considered as containing the most fortu
of his mathematical labours.' 'He proceeds (says his bi
his own course and manner of investigation, and uses his ow
and the great reach of mind displayed in these works seem
acknowledged; but whether he have sufficiently established a
which he considered himself to have proved, remains matte
amongst those most qualified to judge. They were spoken of in
terms of praise by Mr. Davies Gilbert from the chair of th
ciety; but there are some amongst the most distinguished
English philosophers, who still think that his theory of th
too exclusively on analogies, and that many of the elements
putation are too much out of human reach to render the bol
original thought susceptible of being subjected to the severit
matical deduction.'

'Dr. Young, as a mathematician, was of an elder scho
possibly somewhat prejudiced against the system now obtaini
the continental and English philosophers; as he thought th
intellect exercised by a preceding race of mathematicians
small danger of being lost or weakened by the substitution
in their nature mechanical.'

He again visited Paris in 1823, and in the same yea
his 'Account of some Discoveries in Hieroglyphical Lit
Egyptian Antiquities,' in which he gave his own origina
his translations from papyri, and the extensions which h
had received from M. Champollion. This was his first ack
non-professional publication since 1804,—having attain
tieth year, as he states in his preface, and determined t
the shackles by which he had considered himself bound
quette of a medical practitioner.

He made an excursion to Spa and to Holland in 1821, and in this year undertook the medical responsibility and the mathematical direction of a Society for Life Insurance, and declined all participation in the speculation, but had the disinterested satisfaction of witnessing its prosperity. This connexion led him into researches in which he took great interest, and produced his 'Formula for Expressing the Decrement of Human Life,' published in the Philosophical Transactions for 1826; and a 'Practical Application of the Doctrine of Chances,' published in the Journal of Science for October in the same year.

In the previous year he had removed from Welbeck Street to a house which he had built in Park Square, in the Regent's Park, where he led the life of a philosopher, and expressed himself as having now attained all the main objects he had looked forward to in life—of his hopes or his wishes; this end being, to use his own words, 'the pursuit of such fame as he valued, or such acquirements as he might think to deserve it.' In 1827, he was elected one of the eight foreign members of the Royal Institute of France.

With the exception of the consumptive tendency by which his youth had been visited, his health had hitherto been uninterrupted by a day's serious illness. In the summer of 1828 he went to Geneva, and appeared to suffer what was to him an unusual degree of fatigue; on great bodily exertion there was a perceptible diminution of strength, and symptoms of age appeared to come upon him, which contrasted strongly with the freedom from complaints he had hitherto enjoyed.

The Committee of Finance having recommended to the Government the abolition of the Board of Longitude, a bill was passed to that effect, permitting the Admiralty to retain the officer entrusted with the calculations of the Nautical Almanack: this occurred during the time that Dr. Young was abroad, but he continued to execute these duties. Whether the measure was well or ill founded we shall not stay to inquire, but it produced great heart-burnings and discontent amongst those scientific men who considered themselves or their friends treated unhandsomely, as well as illiberally, in the manner in which their services had been dispensed with. It appears that the occasional assistance of men of science was found to be so necessary to many departments connected with the Admiralty, that it was found expedient to form a new council of three members for the performance of duties which had before devolved on the Board of Longitude, and for this purpose Dr. Young, Captain Sabine, and Mr. Faraday, were appointed.

The consequences of this change involved Dr. Young in more labour than his declining state of health rendered him competent to perform without injury, and exacerbated a complaint which must have been long, though insensibly, in progress, but which now was bringing him rapidly to a state of extreme debility. From the month of February 1829, his illness continued with some slight

variations, but he was gradually sinking into greater a weakness till the morning of the 10th of May, when without a struggle, having hardly completed his fifty- He was attended throughout his illness by his friends Dr and Dr. Nevinson. The disease proved to be an ossifica aorta, and every appearance indicated an advance of brought on probably by the natural course of time, nor ev stitutional formation, but by unwearied and incessant la mind from the earliest days of infancy. His remains wei in a vault in the church of Farnborough, Kent.

It has been truly said of this extraordinary man, that cian, a linguist, an antiquary, a mathematician, scholai losopher, he has added to almost every department of hur ledge that which will be remembered to after times. In tl eulogy pronounced by Mr. Davies Gilbert from the Ch Royal Society it is observed, that 'he came into the world fidence in his own talents, growing out of an expectation lence entertained in common by all his friends, which e was more than realized in the progress of his future life. tiplied objects which he pursued were carried to such an e each might have been supposed to have exclusively occup powers of his mind; knowledge in the abstract, the mo generalizations, and the most minute and intricate de equally effected by him; but he had most pleasure in appeared to be most difficult of investigation.' 'The exa Mr. Gilbert) is only to be followed by those of equal per the concentration of research within the limits of some defi of science, is rather to be recommended than the en embrace the whole.

Dr. Young's opinion on this subject is stated by his bio have been, 'that it was probably most advantageous to ma the researches of some inquirers should be concentrate given compass, but that others should pass more rapidly wider range—that the faculties of the mind were more exe probably rendered stronger, by going beyond the rudi overcoming the great elementary difficulties of a variety than by employing the same number of hours in any one that the doctrine of the division of labour, however ap material product, was not so to intellect, and that it went the dignity of man in the scale of rational existence. H it impossible to foresee the capabilities of improvement i ence, so much of accident having led to the most impo coveries, that no man could say what might be the co advantage of any one study rather than of another; and t would scarcely have recommended the plan of his own as of those of others, he still was satisfied in the course whi pursued.'

It has been said that the powers of the imaginat

the only qualities of which Dr. Young's mind was destitute; the writer of this memoir thinks this want at least doubtful from the highly poetical cast of some of his early Greek translations, and is of opinion that it might with more justice have been said 'that he never cultivated the talent of throwing a brilliancy on objects which he had ascertained did not belong to them,' and that his entire devotion to the simple truth, on all occasions, made him averse to the slightest degree of exaggeration, or even of colouring; and that, whether gifted or not with imagination, Dr. Young would, on principle, have abstained from its indulgence.

In all the relations of private life, Dr. Young was as exemplary as his talents were great, and his whole career was one unbending course of usefulness and rectitude.

*** To this Memoir is appended a complete catalogue of all the Works and Essays of Dr. Young, from memoranda in his own hand-writing, for which we hope to find space in a subsequent number of this Journal.

Proceedings of the Academy of Sciences at Paris.

Agricultural and Rural Economy.

Improvement in the Breed of Cattle.—On the 12th of September, M. Silvestre, in the name of himself and M. Husard, made a report on a memoir of M. Girou de Busaringues, on the amelioration of the breed of sheep, oxen, and horses. The reporter, after enlarging on the importance of the subject, and the sagacity and experience of M. Girou de Busaringues, said, that the memoir in question very properly confined itself to results instead of speculations. With respect to increasing the size of cattle, the author proves that the height which it is expedient to endeavour to make them attain, should be in proportion to the pasture peculiar to the country. He then establishes that it is better, in almost all cases, to keep cattle in the fields, and not nourish them in stalls; and afterwards expresses an opinion that the fineness of the wool of sheep is in an inverse proportion to the height of the animal, but in this opinion the reporters do not agree with him, as they consider the fineness of the wool to be dependent on totally different causes. The author then points out the different parts of the various animals to which the peculiar attention of the cultivator should be directed. Thus in sheep the greatest possible length should be given to the body, that being the part from which the wool is derived. In horned cattle, the increase of the production of milk is the most essential thing; while, in horses, regard must be had to the purposes to which they are to be applied. The author's observations, in the present memoir, are confined to the race-horse, and are merely a resumé of the opinions which he has before given to the world, in his work entitled 'Etude de Physiologie appliquée aux Chevaux.'

In pointing out the means of producing the desired am
form, the author remarks that, in horses, it is the male w
transmits more particularly the exterior form, especially
lower extremities; while the internal organs, and the e
of the carcass and crupper of the female, are more genera
in the young. Hence in endeavouring to improve a na
is of more importance to cross it with well-formed stallio
mares. In copulation, the mare should be at least as
horse, and arrived at full development; and the author
a variety of experiments made both on sheep and hor
antiquity of the race exercises considerable influence on
of production; so that in crossing a native breed by co
genous mares with foreign stallions, it is better to select
as have no distinctive mark of race, as the author has
perienced that the foals of mares of an old Navarrois
invariably possessed the characteristics of that race, althou
were covered by a fine Arabian stallion. This observati
peculiar to the author, accounts for the jealous care wit
Arabs preserve the pedigree of their mares, and also for t
which has been experienced in deriving as much advan
been expected from the importation of Arabian stallions.
on the food and health of the animals, with which the n
cludes, does not materially differ from that of other goo
the subject. The reporters concluded by recommending
as far as it goes (being but a slight sketch) to the warm
of the Academy.

Sand Manure.—On the 19th of September, M. Ch
name of himself and M. Silvestre, read a report on a n
sented by M. Dutrochet, at the last meeting, and entit
Sable Silicieux comme Substance fertilisante.' The eart
support of plants; air, water, heat, and manure, are t
principles which stimulate the action of their organs. A
is generally formed of a mixture of four primitive earths,
proportions of which constitute the difference of soils, bu
which would be alone sufficient to constitute good a
Chemical analysis has informed us of the proportion in
earths ought to be mingled in order to constitute a good
is of the highest importance to agriculturists that they s
themselves of this information, in regulating and impr
ground. In the best soils silex is predominant; in the
of the banks of the Loire, it forms forty-nine per ce
found it sixty per cent. in the best soils of England; a
mentions that it is as high as seventy-nine per cent. in a
soil in the neighbourhood of Turin. The experiments
trochet have confirmed the advantage of employing sili
in certain earths. He covered an argillaceous field with
vescent pit-sand (*sable de mine*), and obtained from it n

abundant crops than from similar fields which had not been prepared in the same manner. M. Dutrochet has not contented himself with the mere relation of facts, but has, with great ingenuity, accounted for them by tracing the fertilising qualities of silex to the manner in which it renders the roots of the plants accessible to the air and water, from which they derive their principal nourishment. The reporter, in conclusion, submitted that the memoir deserved the high approbation of the Academy—an opinion which was unanimously adopted.

Botany.

Respiration of Plants.—On the 11th July, M. Dutrochet read a memoir on this subject. He commenced by referring to the experiments of Bonnet and Adolphe Brogniart, from which it had been ascertained that the lower surface of leaves contains a number of aërial cavities, communicating with the external air of the openings of the *stomata*, which, it was presumed, were instrumental to the reception of the principles producing that elaboration of the sap in the leaves which has induced some physiologists to consider leaves as the lungs of plants. M. Dutrochet's object was to verify this supposition. Having observed that many leaves, particularly those of leguminous plants, lost the whitish tint of their lower surface on being plunged into water, he was induced to suppose that this phenomenon might be occasioned by the introduction of water into the cavities previously occupied by air. To ascertain this, he put a bean-leaf into a glass vessel filled with water, and, having completely submerged the leaf, he placed the vessel under the receiver of an air-pump. As the air became exhausted, bubbles of air were seen to issue from the leaf, particularly from every point of the lower surface. After the lapse of half an hour the air was re-admitted, and the lower surface of the leaf instantly lost the whitish tint, which it had preserved until that moment. On taking the leaf out of the water, he found that the lower surface had, in fact, become precisely of the same colour as the upper surface; thus proving that the white tint of the lower surface of the leaf is entirely produced by the presence of air. Sometimes leaves appear to have white spots on the surface: these are proved also to result from the same cause, and disappear as soon as the air is removed by the action of an air-pump. The introduction of the air into the parenchyma takes place through the openings of the *stomata*, which does not prevent those openings serving at the same time for the transpiration of the leaves and the absorption of atmospheric air. M. Dutrochet then ascertained, not only that there is an immediate and easy communication between these aërial cavities by means of all those parts of the leaf which are not separated by thick nerves, but that the aërial cavities of leaves form part of a pneumatic system, extending throughout the whole plant. To prove the direct correspondence between the aërial cavi-

ties of the leaves and the aërial canals of the petiole, M.
plunged a leaf of the *Nymphæa lutea* into a glass vesse
water, leaving the severed extremity of the petiole out o
On placing the vessel under the receiver of an air-pun
hausting the air, he did not see any air issue from the
parts of the leaf; and on re-admitting the air into the ves
ter of an hour afterwards, the lower part of the leaf retaine
tint, which proved that it had not lost the air which in
state filled its aërial cavities. M. Dutrochet then recom
experiment with the same leaf, taking care to submerge
and as soon as he began to exhaust the air, numerous
escaped from the severed extremity of the petiole, but
the edge or border of the leaf. When, however, the air
mitted, a quarter of an hour afterwards, the lower surface
instantly lost its whitish tint and became as green as the
face; thus proving that the air had entirely escaped fron
cavities through the petiole, and that these cavities had b
with water.

The hair or nap which is frequently found on leav
larly the lower surface, is considered by M. Dutroche
filled with air, and as forming the respiratory conduits o
over which they are placed, while the *stomata* are seen i
vals left between those nerves. In the rose-laurel the lo
has no *stomata*, but is covered entirely with nap or hair.

The aërial tubes of the *Nymphæa* are slightly hex
from the angles spring hairs, which, being of the sam
the different united tubes, form for each ternary system c
starry figure noticed by M. Amici. M. Dutrochet con
hairs as conduits which, by their capillarity, absorb the ai
in the canals and carry it into the vegetable tissue, in the
ner as the ramifications of the trachea in insects do into e
the living animal. Other plants offer different dispositi
are calculated to absorb the air from the aërial cavities a
into the most remote parts of the plant. It results, ther
the experiments of M. Dutrochet, that in every part of
are aërial organs, filled with a gas compounded of oxyge
in variable proportions, but in which the oxygen is al
smaller proportion than in the atmospheric air, which pr
has been absorbed by the interior organs of the plant.
circumstance is observed in analysing the air contained
chea of insects, which proves that their mode of respir
same; that is to say, the transport of respirable elastic
their parts. But the origin of this air is not quite the s
insects derive it wholly from the atmosphere, while the p
rate a considerable part of it in their tissue by the influen
The azote, which is not absorbed in the internal respirat
plants, is necessarily expelled; and, in fact, we perceive t
exhale a great deal of azote while they absorb oxygen,

under the influence of light. Leaves, on the contrary, exhale oxygen when exposed to the solar light, and only respire in the shade or during the night. The oxygen which issues from the *stomata* when the leaf is subjected to the influence of light is only a part of what is produced; the rest passes from the aërial cavities into the conduits of the petiole, and thence into the whole vegetable tissue. As there is a continual production in the green matter exposed to the light, the oxygen as it is formed impels forward that which has been previously formed; and this mode of circulation supplies in vegetables the place of that which is produced in animals by muscular contraction.

M. Dutrochet concludes by proving, from a variety of experiments, that as the respiration of plants is supported both by the oxygen which is contained in the air penetrating from the exterior, and by that which is formed internally by a chemical action of the light, (which latter is indispensably necessary to their existence,) the absence of light, by diminishing the irritability of the members of the vegetable kingdom, becomes for them a direct cause of asphyxia, and that plants may, therefore, be hindered of respiration, and consequently killed either by the action of the air-pump or by total obscurity.

Irregularity of Flowers.—On the same day, M. Cassini, in the name of himself and M. Mirbel, made a report on a memoir by M. Adolphe Brogniart, on the relative insertion of the different parts of each floral verticillus, and on its influence on the regularity or irregularity of flowers. According to a modern theory, almost universally adopted, a flower is composed of several verticilli of foliaceous organs superposed and immediately approaching each other, so that the outward or lower verticillus forms the calyx, the next one the corolla, and so on for the stamina and pistils. M. Brogniart, in examining this theory, was led to inquire whether each of these successive rings is in reality a perfect verticillus, that is to say, whether all the pieces which compose it are exactly of the same height round their axis. He remarked, that the calyx of the *helianthemis*, of several caryophylli, &c., have evidently some folioli of the calyx situated more externally, and consequently lower than the others. The manner in which the petals cover each other in many flowers during preflorescence, that is to say, before they are fully opened, also appears to him a proof that these pieces are inserted at different heights. Thus, according to M. Brogniart's opinion, the similar organs which form each of the rings in most flowers do not form exact verticilli, but are disposed at different heights round the axis, or short branch, which bears them; and as the lower pieces are necessarily the outermost, it follows, that their mode of envelopment must indicate their primitive insertion. Thus the imbricated preflorescence, which presents the pieces of the corolla lapped over each other, indicates their disposition in alternate order, while the

valvary preflorescence, in which no piece laps over anc
mately, indicates the disposition in form of verticillus, as p
also the returned preflorescence, in which each piece
neighbour on one side, and is lapped over by that on th
Although uniformity is necessary in the arrangement in f
cilli, it is by no means so in the alternate disposition of the
any more than in that of leaves; and M. Brogniart points
modes of alternation, the most frequent of which is, wh
pieces forming the calyx, or corolla, are disposed on a spi
more than one turn and a half; this is the quincuncial pre
corresponding to the disposition of real leaves in quin
Brogniart admits that the different modes of preflores
corolla are not always the same in the same plants, and
fore, in order to trace the theory of original insertion fr
florescence, it is necessary to regard only the most usual c
and take no notice of the exceptions, a rule generally n
natural history, and which proves the importance of being
our guard against that spirit of system which will only a
lute theories.

M. Brogniart, however, endeavours to account for th
of preflorescence by conjectures, more or less plausibl
principally on the inequality of growth of the different pi
corolla. It is this irregularity which generally constitut
gularity of the flower, and on this M. Brogniart founds
law, which is important and worthy of attention, as esta
interesting relation between the mode of preflorescence an
larity or irregularity of the flower.

If a family of plants presents a valvary or returned pref
both of which indicate a disposition exactly verticillated,
will scarcely ever present any irregular flowers, nor wil
found near it any other family derived from the same prototy
tinguished only by the irregularity of the flowers. If, on th
the preflorescence be imbricated, which indicates an alter
sition, the same family will often contain regular and
flowers; as, for example, in the Ranunculi, or else clo
family of regular flowers will be found another family
essential difference, except the irregularity of its flowers.
Leguminosi may be considered as Rosaceæ, with irregular fl
Fumariæ as Papavera with irregular flowers, &c.

This law, however, is not without exceptions, as the Lo
Synantherus, the Aristolochiæ, have irregular flowers,
their preflorescence is not imbricated, and the floral pieces a
quently inserted at the same height.

M. Brogniart next attributes the irregularity of flowers t
position of the organs of plants to assume a different gro
placed at different heights on their axis, a disposition whic
manifested by leaves, and the existence of which the reporter
by no means improbable. The author concludes his m

applying his theory to the abortion of the *carpillis*, or parts of the pistil, which he also imagines to be disposed spirally, and which would be reduced to three or two, according as the outward or inward part of the spiral is most disposed to increase in growth. The reporter remarks, that these last considerations are too conjectural, and must be regarded as more ingenious than solid; and adds, that the memoir has, in general, too great a tendency to hypothetical views, although it is but justice to acknowledge that the conjectures of M. Brogniart, which are, as usual, ingenious and interesting, are not allowed to usurp the place of positive facts, an abundance of which will be found to constitute the most solid part of the memoir. In conclusion, the reporters recommended the memoir to the warm approbation of the Academy.

Development of the parts of Plants.—On the 18th of July, M. Cassini read a report on a memoir by M. Barbe, entitled 'Observations sur l'impulsion qui provoque la saillie des germes radicaux adventifs, et sur quelques autres points de Physique Végétale.' The object of the memoir, which will form part of a large work hereafter, is to account for the fact previously observed by M. de Mirbel, that the adventitious radical germs always develope themselves in a horizontal direction; or, to speak more correctly, in a perpendicular or right-angular direction, with reference to the root from which they spring. M. Barbe has given them the new name of *éradians*, because they are under the influence of a powerful radiating force; and concludes that the sprouting of the adventitious radical germs cannot be produced by the ligneous fibre, because that could only act in its own direction, which is always parallel to the axis, but that it is operated on the points of the medullary radii, nearest to the *liber* or inner bark; and he is of opinion that the young roots are only expansions of these radii. In reply to the objection, that the roots are produced on the substance of some leaves, (as for instance, the *cardamine pratensis*,) M. Barbe states, that there are also medullary radii in the leaves; in proof of which, he cites the horizontal section of the petiole of the leaf of the orange-tree, as a proof of there being no connexion between the production of the adventitious roots and the development of the foliaceous buds. M. Barbe mentions the singular fact of a slip of willow having produced several roots, already arrived at a ligneous state, and even a new bed of wood and bark interposed between those which existed at the time of plantation, although the foliaceous buds were not at all developed. M. Barbe has also particularly examined the functions of the small spots or rents in the epidermis, which M. de Candolle calls *lenticelles*, and supposes to be buds of the roots; whereas M. Barbe maintains that, if they be not vegetable parasites of the class *cryptogamia*, (which, however, he is disposed to think they are,) they are merely eruptions of the feculent cellular tissue accumulated under the cuticle, and have no relation whatever with the production of the radical germs. The

memoir also contains allusions to various other points, be treated in M. Barbe's forthcoming work; but th developed with sufficient minuteness or clearness to ad being examined. The reporter, however, considered th deserved the approbation of the Academy, as an encou continue his researches on a subject highly important to

Fossil Plants.—On the 18th of July, M. Beudant ma to the Academy, on the publications and manuscript me Adolphe Brogniart, on this subject. M. Brogniart has lished five numbers of his work, containing thirty-one she press and seventy lithographic plates, representing a grea fossil plants, compared with the analogous living plan juxtaposition with them. In studying fossil plants, a v system from that in which a knowledge of living plants must necessarily be pursued. The organs of fructificatic botany, supply the characteristics of the primary class, invariably deficient in fossil plants; and the leaves and s in living plants are usually the object of a very superfici all that remain. M. Brogniart has, therefore, especi himself to examine minutely the organization of these p view of appreciating with exactness the relation of the ch to be drawn from them, with those derived from the o usually serve as grounds for classification. He has ca mined the nerves of the leaves, and the different mode of these organs in each family; he has investigated the n which the different parts of the internal structure of pla duce on their exterior, and has, in a word, applied him most minute researches on every characteristic of living p could by any possibility be preserved in those buried in th the earth. He has also endeavoured to ascertain the diffe cations, which compression and the various modes and decomposition might produce in fossil plants; and has d with the greatest precision the cases in which the family and even the species of a plant, may be pronounced o tainty, from those in which doubts may be entertained these points, and it may, therefore, be only safe to de genus, the family, or the class of a plant. By these Brogniart has succeeded in distinguishing among fossil cies, genera and families, perfectly analogous to thos plants; and has already disposed more than five hund of these plants in their natural order, and, by comparing living plants, enabled us to appreciate the immense distin isting between the ancient and modern botanical world. niart has not stopped here, but has proceeded to examin regulating the distribution of fossil plants in the different earth, and compared these with the laws of the geographic tion of plants on the surface. He has ascertained with

that the vegetable remains found in the most ancient deposits of the globe are plants belonging specially to the families of the equisita, ferns, and lycopodia; that it is not until a higher series of formations about the mottled freestone (*grès bigarré*) that some coniferæ are found ; the cycades are only found still higher, and the dicotyledonous plants do not appear until immediately after the chalk. The primary vegetation of the globe, therefore, consisted of vascular cryptogamiæ, with about one fifteenth part of their number of monocotyledonous phanerogamiæ, and these plants were of immense size ; the horse-tails or equisita grew to ten feet in height and from five to six inches in diameter ; the ferns from 40 to 50 feet in height, and the lycopodia from 60 to 70 feet. When the coniferæ begin to appear, the cryptogamiæ become less numerous ; the species are no longer the same, nor have the same magnitude. When the remains of cycades commence, the species of the cryptogamiæ are again different, several genera have entirely disappeared, and the number of these plants, which in the first epoch bore a proportion to that of the monocotyledonous phanerogamiæ of 14 to 1, is now only about equal to it : so that in the primitive floral world the cryptogamiæ formed $\frac{14}{15}$ of the whole number of plants ; in the middle epoch they constituted only half, and the other half was composed of coniferæ and cycades ; while in the present distribution of plants on the surface of the earth these families scarcely form one three-hundredth part of the whole number. When the dicotyledonous plants appear in the beds of the earth, their number becomes suddenly immense, and the cryptogamiæ, which then belong to genera different from those found in the former beds, disappear almost entirely. The numerical relation of the different families with each other then becomes nearly similar to that of the plants now existing on the surface, and the most numerous species are those which have the strongest analogies with living plants. Hence it appears that the vegetation of the earth has greatly changed at different epochs, and has become more and more complex, so that the long lapse of time, during which all the deposits have been formed, is divided into various periods of unequal length, during each of which vegetation had peculiar characteristics uniform throughout the earth, and after each of which vegetation completely changed in its genera, its families, and even its classes, as well as in the numerical relation between the different species, until it ultimately arrived at a point nearly resembling that which now exists. It is also remarkable that the beds in which are found the remains of plants of any given period are separated from those containing the remains of plants of a different period by deposits entirely destitute of terrestrial plants ; whence we may conclude that there were periods of repose, during which the surface of the earth was wholly unproductive, a circumstance which has an immediate relation with the differences observed between the various periods of vegetation, which indicates the sudden convulsions by which every thing then existing was destroyed, and

which shows us at each change a new order of things
harmony with the subsequent atmospheric circumstance
portance of these considerations, when united with the
recently made on the fossil remains of animals, and the
of M. Elie de Beaumont on the relative ages of the *n*
crystallization, will be obvious to every geologist. M. B
also gone into some curious researches on the compa
laws regulating the different epochs of vegetation in
world with those now existing. The vegetable remains
period have much greater resemblance to those now fou
equator than to those of the temperate zone. In the
we find much higher species of horse-tails, ferns, and
than are observed near the poles: many of them are
rescent there. But the size of the fossil plants of those
the first period of vegetation is much greater than e
those now existing at the equator, consequently the c
velopment were then much more intense than at prese
other hand, M. Brogniart remarks that these vascular c
formed $\frac{9}{10}$ths of the primordial vegetation, whereas t
constitute $\frac{1}{30}$th of the actual vegetation; there must, the
been some cause producing this preponderance. It had
remarked that these cryptogamiæ attain the greatest size
tropics; but M. Brogniart is the first to call attention
that their relative quantity is greatest in islands, and be
considerable in proportion as the islands are smaller a
tance from the continents, so much so that in some s
they are even now almost equal in number to the pha
whence we may conclude, that if the world were comp
continents, but of small islands only, the cryptogami
equal or superior in number to the phanerogamiæ. T
tion is of great importance when considered in conjuncti
geological facts which induce us to conclude that the ex
nents were not always in their present state, but have l
piecemeal, and that probably, in the most remote ages, th
sisted only of islands and archipelagos in the midst of
M. Brogniart then shows us, that, in the subsequent peri
tion began to resemble that which we now see on the c
the large equatorial islands, both in the size of the plants
to which they belong, and the numerical relation between
lies. Finally, he shows that, in the most recent period
pearance of the dicotyledonous plants, the vegetation be
gous to that now existing in large continents. These obs
derived from the general examination of all M. Brogni
and manuscript labours. The work in course of publicat
'*L'Histoire des Végétaux Fossiles*,' will form a complete
the fossil plants considered with a view to their classificati
analogy with the plants now existing. The five number
appeared, treat of the confervæ, the fuci, the equisetæ,

mosses, and some genera of the family of ferns. Each family is preceded by general considerations on the characteristics of the existing plants which it contains, on their anatomical organization, their generic divisions, and their distribution on the surface of the earth. The fossil plants of the same order are then compared with these, and the geological circumstances under which they are found minutely and accurately described. The anatomical structures of the families of the equiseta and ferns, particularly the organization of the petioli, and the characteristics of the stems of the latter, are, for the first time, fully and accurately described, and all the plates and tables of the localities in which the different fossils are found are remarkable for their scrupulous fidelity. In conclusion, the reporter observed that the philosophical spirit in which the researches of M. Brogniart had been carried on, and the importance of the facts which he had collected, as well in a geological as in a botanical point of view, specially merited the highest and most signal approbation of the Academy.

Structure of the Stems of Plants.—At the meeting of the 25th of July, MM. Desfontaines and Cassini made a report to the Academy on a memoir by M. Adolphe Brogniart, entitled 'Observations sur la structure et la mode d'accroissement des tiges dans quelques familles de plantes dicotylédones.' The object of this memoir is to supply the deficiency in science observed by M. de Candolle, who has remarked that the classification of plants can never be considered as truly according to nature, until the characteristics derived from the organs of vegetables can be made constantly concurrent with those derived from the organs of reproduction. M. Brogniart endeavours to attain this end by proving that several families of plants have distinctive and peculiar characteristics, observable in the structure of their stems. After alluding to the general difference arising from the fact of the plants being *monocotyledons*, or *dicotyledons*, the author remarks that, in the greater part of the ligneous dicotyledons, the fibrous fascicules, disposed in concentric circles, which form the successive beds of wood and *liber*, approach each other laterally, and are united at various distances; so that the radii, or rather the medullary plates which separate them, have their length frequently interrupted by reticulations. This disposition, although general, is not universal: thus, in the vine and the *cimis*, the medullary radii form very long continuous plates without interruption. M. Brogniart has made the same observation in all the menispermæ which he has been able to notice, as well as in the ligneous ranunculi, such as the *clematis*, in the *aristolochiæ*, and in the *pipera*. It might be hence concluded, that the structure belonged generally to creeping or climbing plants; but the author, not having found it in the *Bignonia*, *periploia*, honeysuckle, ivy, &c.; and having observed it in the *berberes*, is induced to suppose it a characteristic, independent of the manner of growing, and having relation

to the organization peculiar to certain families. Thus, t continuity of the medullary radii, from one articulation may be considered one of the distinguishing character plants above mentioned. With the exception of the *aristolochiæ*, which show it clearly at every age, this can only be observed in the young stems. M. Brogn marks, that the greater part of climbing plants have the entirely composed of vessels of so large a bulk as to be naked eye on the transversal section, which appears full a sieve. This characteristic, which is peculiar, not to th to the mode of growth of the plant, is considered by th necessary consequence of the small volume and great l stem, which bears numerous leaves of a large size, b leaves occasion a profuse transpiration, and therefore rec ture of stem which will admit of a rapid passage of the s the loss occasioned by the transpiration. M. Brogniart the remark which he has made on the internal organi vine, the *clematis*, and the *aristolochiæ*, the most i which is the elliptical or oblong form of the section o the last named family. The author also establishes menispermæ, the *liber*, or fibrous part of the bark, al in the same state that it was in during the first year, n in any manner. Thus the *cambium*, which is interpo the wood and the bark, instead of being divided as us beds, the one of wood and the other of *liber*, is here e to the ligneous body. He has also remarked that, in structure of the stem of the menispermæ, the ligneous bc into several concentric layers, perfectly distinct from, and of each other, each of which is the produce of several ye the new beds are frequently entirely wanting on one stem, which is thus left naked. The *pipera* also offe markable characteristics, particularly, 1st, the formation ous fascicules between those which primitively composed ring surrounding the pith, by which the diameter of t canal is increased, and 2ndly, the existence in the interior fibrous fascicules analogous to those of the ligneous body of which increases every year. M. Brogniart remarks, i that comparative anatomy ought to be the basis of the of vegetables, as well as of that of animals; and that the teristics of the organization of a plant are rather to be body of the stem which produces the leaves and flowers, leaves and flowers which are but the offspring of, and the stem. The reporters, although not disposed to bel internal analysis of the organs can ever be of the same i botany as it is in zoology, agree with the author that it be a source of interesting and important investigation. concluded with the highest eulogium on the talents an M. Brogniart, and the recommendation of the insertion

in the 'Recueil des Mémoires des Savans Etrangers,' which was ordered by the Academy accordingly.

Fecundation of the Orchideæ and Cisti (Cistinus).—On the 1st of August, Messrs. Cassini and Auguste de St. Hilaire made a report on a memoir by M. Adolphe Brogniart on the above subject, of which the following is an abstract. The object of the memoir is to explain the mode in which the pollen acts on the stigmata, and the manner in which the fecundating fluid passes thence to the ovary in these two families of plants, the reproductive organs of which are not formed in the usual manner, with a view to strengthening the theory formerly promulgated by him as to the general mode of fecundation of plants. The pollen of the orchis is agglomerated in divided and subdivided masses in such manner that the last groups are composed of three, four, or five spherical grains. When these masses fall on the stigma, some of the grains are separated from the rest, and fix themselves on that organ. Each of these grains soon produces a membranous tube, which penetrates into the stigmatic tissue, formed of utriculi, elongated, free, and only united by a viscous liquid.

In the epipactes, the pollen is pulverulent, formed of small aggregations of four spherical grains, which remain always united, and which, when they fall upon the stigma, give birth to very long tubular appendages which penetrate deeply into the stigmatic tissue.

The ovary of the Orchideæ offers internally a simple cavity, having three longitudinal projections, each divided into two laminæ; these are the placentæ, which have on their edge an infinity of ovula, so disposed, that the opening through which the fecundating fluid ought, according to M. Brogniart's theory, to reach them, is diametrically opposite to the point by which they are attached to the plant. This appeared an astounding objection to the theory, but M. Brogniart explains it thus. The stigmatic tissue is continued in the axis of the column which constitutes the style; it is there, at the summit of the cavity of the ovary, divided into three *faisceaux*, each of which is subdivided into two filamentary bands, which are applied to the two laminæ of each placenta, and as the separate filaments which form these bands are twisted or folded back in festoons, which penetrate between the ovula, and often appear to extend quite to the orifice at their extremity, the stigmatic tissue serves as a conductor to the fecundating fluid, which is thus enabled to attain the orifice at the extremity. The family of the cisti (*cistinus*) offers, from the ordinary position of the orifice of the ovula, the same objection to the theory of the fecundating fluid proceeding from the stigma to the interior of the ovary by means of that orifice; but M. Brogniart remarks, that when the orifice of the ovula is opposite to the point of junction with the plant, these ovula are placed on a very long umbilical cord, which is tortuous or bent back, so that the disengaged and open extremity of the ovulum is in contact with the sides of the ovary, of the partitions, or of

the placentæ, a fact which is quite sufficient to establish the pos bility of the communication, as the stigmatic tissue, the conductor the fecundating fluid, could very easily creep along the sides of ovary until it came in contact with the orifice. And, in fact, it quently happens, that in some Helianthema, the umbilical cords which are not so regularly bent back as to bring all the ovula in c tact with the placenta, a number of ovula, which have become a tions, are found in the fruit when ripe, their vestiges remaining persed among the perfect grains. The following facts, pointed by M. Brogniart, are still more remarkable.

In the *Helianthemum lævipes et thymifolium*, the ovary con six ovula, attached near the summit of its cavity to the three centæ by cords so short as to be unable to bend back. But in case, the orifice of the ovulum, instead of being, as in the species, opposite to the point of junction, is almost close to it, prolonged in a small tube which is applied exactly to the base style at the point of termination of the extremity of the cond tissue. In other species, such as the *Helianthemum Ægypt et Niloticum*, where the numerous ovula are inserted on pla which are parietal, not projecting, and supported by cords straight, so that their open extremity cannot be brought in tact with any point of the internal sides of the ovary, M. niart has discovered that the conducting tissue, which occu axis of the style, is prolonged downwards to the middle of th of the ovary in a bundle, divided and subdivided into a nu fine and floating filaments, which can easily carry the fec fluid to the orifice of the ovula. From these facts, M. B concludes, that whatever may be the structure of the pollen, always produce a long membranous tube, which penetrates the utriculi of the stigma to deposit the fecundating matt tissue, and that, notwithstanding any modifications in the of the ovary, the stigmatic tissue or conductor always pen it, and is there disposed so as to be placed, by some means in contact with that part of the ovulum which, being op of the introduction of the fecundating matter.

On the truth of this theory, however perfectly it may be supported, it is not the province of the reporters to p but the facts adduced have been verified by them, and fo equally new, interesting, and exact; and the importanc facts is sufficient, independent of any hypothesis, to entitle t to be inserted in the 'Recueil des Mémoires des Savans E

Abortions and Irregularities of Flowers.—On the 1st Messrs. Cassini and Mirbel made a report on the Memoi Jussieu on this subject, of which we gave an account Number (page 133). After having analysed the m reporters remark, that by a singular chance M. Adolph had examined nearly the same question as M. de Jussieu

time, and arrived at a similar result. (Vide Report, July 11th, on his Memoir, page 338.) While the memoir of M. de Jussieu was under the consideration of the reporters, he forwarded to them another and more detailed memoir, containing a great variety of interesting observations and illustrations of the family *Malpighia*. The ovula of this family are different in their mode of development from all those with which we are acquainted; they cannot, therefore, be referred to either of the three classes, *Orthotropes*, *Anatropes*, and *Campulitropes*, as they appear to participate of all these forms. The ovula of the *Hippocastana* are entirely *anatrope*. Two in each ovary, they present the remarkable characteristic, that in developing, one breaks out (*se renverse*) from top to bottom, and the other from bottom to top. The ovula of the *Acerinus* are also anatrope, but not so decidedly so as those of the *Hippocastana*. The *Camarea hirsuta*, and the *Camaria affinis*, produce two kinds of flowers, the one conspicuously situated at the upper part of the stem, having four large petals, six well-conditioned stamina, and three fecund pistils; the other very small, hidden in the angle of the lower leaves, having no corolla, but one *indehiscent* stamen, no pollen, and two ovaries, generally without style or stigmata, yet producing good seeds like the ovaries of perfect flowers. The father of M. Adrien de Jussieu, in his *Genera Plantarum*, had divided the family *Malpighia* into two secondary groups, characterised the one by pulpy, the other by winged fruits; but M. Adrien de Jussieu remarks, that there are genera with capsular fruits, which belong to neither of these sections, and form a transition from one to the other. M. de Candolle had added a third group, under the name of *Malpighia Monostyla*; but the supposed single style is, according to M. Adrien de Jussieu, nothing but a bundle composed of several styles fastened together, and proceeding from an equal number of united ovaries; and the genus in question contains species, in some of which the styles are united quite to the summit, while in others, they are separated almost down to the base. The author, therefore, rejects both these divisions, and considering that the genera are only distinguished from each other by small and graduated differences in the degree of abortion, he prefers considering the family as a whole, without establishing any artificial division into tubes or groups. The memoir details the particulars of sixteen known and eight new genera, in which he has added to the 195 species before described, 90 hitherto unknown, or not supposed to belong to the family. The author avows his intention of re-arranging all the botanical families; and if immense materials, indefatigable research, and acute observation be sufficient to enable him to do so successfully, he is sure to triumph. If M. de Jussieu's theory of organization be open to some doubts, his efforts to dispel those doubts have been highly advantageous to science, as they have elicited a number of curious and authentic observations on the floral organs of the *Malpighia* and kindred families.

Family of the Chenopodia (*Chenopodées*).—At the sa M. Auguste de St. Hilaire read a report on a memoir b Moquin on this family,—one of the least known in tl world. This memoir, which is the first of a series, is de examination of the genus *Sueda* and the other *Chen* allied to that genus. The genus *Sueda* had been conf the *Chiropodium* and the *Salsola*, until Forskal propo them in a separate group under the above name; but i until now, been accurately described by any naturalist. with ligneous or herbaceous stems, and fat and succ almost vermicular or cylindrical, grow on the borders of of lakes; they will always afford soda by incineration; presence of this substance in the tissue is accidental, it wi when the plant is cultivated at a distance from salt water M. Moquin enters into very long and minute descript various organs of the plant, and affords some remark tions of anomalies in different species, particularly the the *perisperma* in the *Atriplex*, the *Beta*, and the *Ch* and its non-existence in the *Salsola*, the *Camphorosma basis*, &c. 'The species of liquor,' says M. Moquin, 'i of which the embryo of the *Salsola* at first floated, becon absorbed by it. When this embryo has attained its full is larger or longer than that of the *Chenopodia*, which abundantly albuminous; it is more advanced, and has the colour of a little plant. Consequently a seed of *Chenopo* has no perisperma, only differs from an albuminous s same family, inasmuch as it has already absorbed its p nourishment, and its embryo is rather more advanced in i It results also from this observation, that the moment c of seeds is not in all plants precisely that at which th have attained the same degree of development. Thus seed of *Sueda*, having a spiral embryo, but without peri not analogous, as to its growth, to a grain of *Anserina* whi left the parent stock; the latter at its maturity resembles *Sueda*, which is still at a certain distance from maturity. *Chenopodées* which have a perisperma the embryo is white which have none it is greenish. The *Sueda*, however, tions to this general rule, as their embryo is white, and the trace of perisperma. The reason of this, according to M is, that the *Chenopodia* which have a perisperma, general double integument, the thick and crusty exterior of which passage to the light, and the embryo therefore remains white the others have only a simple, membranous, thin tunic, the which allows the passage of the rays of light, which pro green colour. The *Sueda*, although without perisperma, exterior crusty integument like the *Anserinæ*, and the emb fore does not become coloured.

The genus *Shanginia* (which, as well as the genus *Schob*

first distinguished as a separate genus by M. C. A. Meyer) forms a singular exception to the rest of the family of the *Chenopodia* in having a *semi-infère* fruit. M. Moquin supposes the adherence of the pericarpium to be owing to an intermediate discus between the ovary and the calyx. The reporter, without rejecting this explanation, remarks that the consequence deduced is not a necessary one, as many plants present a large discus joined to the calyx, without having the ovary otherwise than free and disengaged.

The *Sueda*, the *Schoberia*, and the *Shanginia* form a little tribe in the family of the *Chenopodia*, the distinguishing characteristics of which are a white or whitish embryo, spirally turned, usually without perisperma, and always surrounded by a double integument, the exterior of which is crustaceous. The various plants comprising this tribe are described by M. Moquin with great accuracy and minuteness; but it is obviously impossible for us to follow him into this detail. The reporter observed that the work merited the full approbation of the Academy, and urged the author to continue his researches on the other tribes of the same family*.

Decoloration of Leaves; Vegetable Nutrition.—On the 8th of August, M. Dutrochet read an elaborate memoir on the above subjects, of which the following is an analysis. It is well known that when light is excluded from any of the vegetable kingdom the leaves lose their green colour, and become of a yellowish-white. The physical cause of this is the loss of carbon, which, when the action of the light no longer fixes it in the tissue of the plant, is poured out into the atmosphere in the shape of carbonic acid, and the plant, deprived of the substance to which it owes its green hue—the sign of life and health—languishes into a morbid paleness. But the loss of carbon is not the only cause of the change of colour; it is also produced by the exhaustion of the soil in which the plants are growing. Thus the leaves of a plant in a garden-pot will, if not watered with manured water, lose their green tint, and become white about three years after they have been allowed to remain in the same mould. Three years is mentioned as an average period, but the time will be greater or less according to the degree of nutritive principle and to the volume of the mould in the pot. An example illustrating this fact occurred a short time since. A gentleman, whose house was situated on a calcareous rock, dug large holes in the rock, filled them with earth, and planted peach trees. These trees had every atmospheric advantage; the fissures of the rock allowed free passage to the rain-water; and from their full exposure to the sun and light there could be no deficiency of carbonic acid; yet, after flourishing a few years, the leaves began gradually to change colour, and ultimately, when, from

* M. Moquin appears to adopt the term *Chenopodia* as indicating the whole family, and in opposition to *Chenopodium*, one of the genera of that family. The same remark applies to the *Cistinus*, as opposed to the *Cistus*, and the *Euphorbiacées*, as opposed to the *Euphorbiæ*, in the preceding report.

the exhaustion of the nutritive principle of the earth in which the were planted, the suction of the roots no longer supplied any alimer tary carbon, the trees produced only white or pale-yellow leaves.

The same phenomenon may be established by remarking in sprit the difference of colour between grain growing in a rich soil, and th growing in a poor soil. The moment the sap which nourishes t plants cannot derive a sufficient quantity of carbon from the eart the leaves become paler and paler, until the carbon is quite exhauste and they are then produced quite white. In the decoloration leaves from want of light, there may be plenty of carbon in the pla but, instead of being fixed in the tissue, it is dispersed under the fo of carbonic acid; while in that arising from the exhaustion of t soil, the carbon, which is the essential colouring principle, is wantin and, therefore, the brightest rays of the sun produce no effect. C is a third cause of the change of colour in the leaves of plants; t results both from the obstacle opposed by the lowness of the tem rature to the nutrition of the leaves, and also from the age of th organs. Those plants which have the greatest vigour of vegetat will always resist the longest the influence of the cold, which te to suspend their nutrition, and, therefore, to change the colou their leaves. This is remarkable in wheat and other grains; the of winter does not affect them; they grow up in the spring with gr leaves; but if, afterwards, cold weather comes, the nutrition is pended, and the leaves become yellow. If, however, in the sa field, some parts have been better manured than others, and, co quently, have stronger nutritive principle, the grain growing on th parts will preserve the colour of the leaves long after that of the o parts has given way to the influence of the cold. Hence we find a certain depression of the temperature occasions in plants, du their development, a suspension of the fixation of the alimen carbon; and, consequently, (as they are constantly losing som their carbon in the form of carbonic acid,) a change in the colo the leaves, but that the effect of this lowness of temperature wi in a great measure resisted by plants which possess in a conside degree strong principles of nutrition.

All these observations tend to prove that it is from the soil plants principally derive the alimentary matter by which they e this alimentary matter is an *extractive*, soluble in water, existin various proportions in the different vegetable earths. It is abundant in the offal of animal and vegetable matter, whence and manure are formed. It appears from the experiments of M Saussure, that the oxygen of the atmosphere combines wit carbon of this *extractive*, and changes it into carbonic acid, w being dissolved by the sap, is transported into the leaves, where decomposed by the action of the light; the carbon is fixed i vegetable tissue, and contributes to form the green colouring m and the oxygen is discharged.

All carbon which is susceptible of being converted into car

acid at the ordinary temperature of the atmosphere, is adapted for the nutrition of plants. This carbon is to be found in the extractive matter which abounds in vegetable earth, and which is also found in solution in all waters, even in the most apparently pure springs. This carbon exists also in an organic matter which is as volatile as the water in which it is dissolved. When water, charged with organic substances, is distilled, water is obtained which is not pure,—it contains a matter which, if not organic, is at least organizable. This water, when exposed to the light, develops and produces the green matter of Priestley, and the *Vaucheria infusoria*, which proves that it contains alimentary matter. When the water contains a considerable quantity of this matter it is sensible to the taste; but otherwise its presence cannot be detected, as it is not affected by any chemical re-agent. The volatility of this organizable matter proves that it must exist in the water which is evaporated from the surface of the globe. When this water falls again in the shape of rain, it meets, in its course through the atmosphere, volatile emanations both animal and vegetable, with which it becomes charged, and thus rain-water, distilled by Nature herself, falls charged with matters adapted for the nutrition of plants. Thus we perceive that, although part of the nourishment of plants is obtained from the atmosphere, it is as much derived from the carbon contained in the rain and dews, as from the absorption of the atmospheric carbonic acid. But it must be admitted that the principal aliment is derived from the soil through the roots. Boyle's experiment on a willow branch which, in five years, increased in weight one hundred and sixty-five pounds, while the earth in the pot in which it was planted had only diminished two ounces, does not prove that the plant was alimented solely or principally by the atmospheric carbon, because it had been from time to time watered with water more or less charged with *extractive* substance. It must be remarked that when plants constantly impart the *detritus* of their leaves to the soil in which they grow, they enrich it with alimentary carbon instead of exhausting it, which proves that a portion of their alimentany carbon is derived from the atmosphere, but that portion would be insufficient for their aliment if unassisted by the carbon introduced through the medium of the roots. It results from this two-fold origin of the alimentary carbon, that the plant in its annual fall of leaves, and in its decomposition after death, communicates to the soil more carbon than it had received from it. Every year, therefore, that a tree lives, it not only enriches the soil in which it grows, but it acquires more powerful sources of aliment, because to the atmospheric carbon, the source of which is always the same, it adds the earthy carbon, the quantity of which is annually augmented. This earthy carbon necessarily becomes exhausted in the fields, when we remove the vegetables which they have produced; thence the necessity of replacing this carbon, or rather this extractive, by the addition of manure. This may also be effected, although with less advantage, by growing herbaceous plants and burying them in

the soil under the plough; these plants, in their decon
return to the soil the carbon which they had borrowed fro
also enrich it with that which they had derived from the atr
It may appear paradoxical, but it is unquestionably true, t
soils furnish too much aliment to plants: thus wheat grow
very rich soil, will have an exuberance of leaves, and the ste
down by their own weight, are bent towards the earth, by v
vegetation is suspended and no grain is produced. Even t
which remain in an upright position produce very little gr
superabundance of nutrition, producing in the plant the sa
as obesity in animals, considerably diminishes its generativ
The cause of this phenomenon must be sought in an exami
the mechanism of vegetable nutrition.

The alimentary carbon derived from the soil is conve
carbonic acid by the oxygen of the atmosphere, which t
absorbs by means of its respiration; this carbonic acid is
prived of its oxygen by the influence of the light, and it is
the alimentary carbon, being set at liberty, assimilates itself
plant which it nourishes. It follows, therefore, that if the e
matter obtained from the soil be so great, that it cannot al
dified by the oxygen of the air, and assimilated by the light a
mentioned, the plant will be gorged with juices imperfectly p
which will be incapable of performing the most importa
organization, that of sexual generation. The nearer the st
to each other, the more this effect on the leaves will be per
since they mutually keep the light from each other, and the i
of light being indispensably necessary to the elaboration of th
tive juices, many of the leaves, particularly the lower ones,
blanched and die. Count Chaptal, in his 'Chimie app
l'Agriculture,' has noticed this phenomenon, and has also re
that when, by excess of manure, a soil has been raised to an ex
state of nutrition, the produce will taste of the manure, whic
that the alimentary carbon has not been properly elaborated
pared: thus the quality and flavour of grapes grown on a vin
has been abundantly manured, will be found far inferior t
grown on a vine which has been carefully and moderately m
We often hear agriculturists remark, that fields of wheat, whi
appeared luxuriant and flourishing in the spring, have belie
promise, and afforded but indifferent harvests; the cause of
now evident to the physiologist,—the plants had derived too ab
a nutrition, and their functions, instead of being properly dev
were smothered and rendered inert. Hence, it is of the great
portance, for practical agriculturists to study well the characte
soil with which they have to do, that they may avoid giving
much, as well as too little artificial nutrition. It is well kn
horticulturists, that trees which exhibit a great luxuriance of
and leaves, rarely, if ever, produce fruits or flowers; this is
result of a superabundance of nutrition, but is produced in a di

manner from that which has just been noticed with respect to wheat. In the latter, the organs of fructification appear, but produce no effect because they cannot perfectly perform their functions; in the trees, on the contrary, the organs of fructification do not appear at all. The flower-buds are, in fact, merely metamorphosed leaf-buds; in order to effect this metamorphosis, it is necessary that the buds should remain a certain time stationary. It is while the bud is in this state of apparent repose, that the formation of the organs of reproduction (which are merely leaves in another form) takes place. In those trees in which the vegetation is too vigorous, the sap is impelled so constantly and so powerfully towards all the buds, that they are never allowed the state of repose necessary to the conversion of the foliaceous organs into floral organs, and consequently are immediately developed into branches laden with leaves. This is the true cause of the absence of flowers in the trees of the tropics, which has been particularly remarked by M. Auguste de St. Hilaire, with respect to the forests in Brasil. This observation explains the means by which horticulturists are enabled to raise fruits out of their natural season, as cherries in January, grapes in April, &c. In order to effect this, it is only necessary to change the period at which the buds remain in that stationary position, which enables them to develope the germs of the floral organs. For this purpose the plants are kept during the winter in a hot-house, where their vegetation is very active; they are then taken out in the spring and exposed to the north, in a situation sheltered from the solar rays. This change of temperature suspends the vegetation, the plants lose their leaves, the buds become stationary, but being in a temperature favourable to the internal vegetative progress, they form the floral organs, which would never have been called into existence, had they continued in the hot-house. After two or three months of this *æstival hybernation*, the plant is replaced in the hot-house, the vegetation recommences; the flower-buds which have been elaborated during its repose become developed, and thus the time of efflorescence having been artificially changed, fruit is produced, which attains maturity at a season different from that which nature has assigned for its development.

The memoir was referred to a committee, consisting of Messrs. Mirbel, Cassini, and Auguste de St. Hilaire.

Night-blowing Flowers.—On the 8th of August M. Auguste St. Hilaire read a report on a memoir entitled 'Flore Nocturne,' by M. Viret. The object of this memoir was to explain the causes of the well-known phenomena of the closing or sleep of plants. M. Viret thus details his theory:—'My experiments have led me to the following conclusions. Cold and wet diminish the transpiration of plants. In that case the sap, instead of ascending towards the top in the branches of the leaves and flowers, as in the daytime, descends towards the roots. Hence the sap-vessels, or the upper parts, which,

in so many plants, are very thin and fragile, remain empt compressed by their natural spring. This is the reason tha compound and syngeneceous flowers, such as the *malvacea volvuli*, &c., close during the night, and even when the sky i When, on the contrary, the sun is radiant in the horizon, and light soon recall an abundant sap into the branches petals of the flowers open. Thus the flowers of the wild when closed by the cold, will, if brought into a warm place if their peduncles are plunged into warm water, so as to occ ascent of the sap, unfold their petals, and resume their vigour.

The same principle accounts for the opposite phenomenon in night-flowers: they are closed in the day, because the light of the sun are too powerful for the frail texture of certai and evaporate too much of the sap and nutritious juices wi their vessels. But in the freshness of the evening, when the juice, not being so powerfully evaporated, rest in greater qua the tissue of the plants, they dilate those vessels, and the flowers r The reporter remarked that this theory is precisely similar to Adamson; but in order to admit that it is strictly true, it n established that the nocturnal plants, which are represented as liable to be too powerfully acted on by the solar rays, are more frail and delicate than those diurnal ones which only fl under the strong influence of the sun. This, however, is not th since the cistus, the aquatic ranunculi, the *celemacea*, the *h charices*, and certain of the cruciform genus, which are d plants, are flowers of extreme fragility and delicacy, and therefore, according to the theory, to have their sap so comp evaporated, that their petals would be closed all day, and only at night. The reporter, however, commended the industry o Viret, and recommended the Academy to request him to contin communicate the result of his experiments and observations o closings of the petals or flowers, as likely to tend to the estab ment of a theory not liable to the objections raised to that now forth. The report was adopted.

Self-fecundation.—At the same meeting, M. Dureau de Lan presented a piece of female hemp, which he believed to have l fecundated without having been subjected to the influence of male hemp. He remarked that, although self-fecundation was a p ciple which could not in any case be admitted, he had seen reaso believe that some plants possessed the property belonging to so insects, of being fecundated for several generations in advance, which means the later generations would present the appearance fecundation without the intervention of a male; whence has aris the idea of self-fecundation.

Flowers of the Resedæ.—On the 5th of September, M. Augus

St. Hilaire presented a memoir on the flowers of the resedæ, of which the following is a summary:—After recalling the observation made by M. de Mirbel on the want of consistency in vegetable organization, and remarking that, in some genera, a particular class of organs will occupy precisely the same place as is occupied by a totally different class in a neighbouring genus, he observes that the family of the resedæ offers a remarkable example of this species of transpositions. After a general description of the flowers, he examines their parts in detail, and remarks that the petals in the bud are at first perfectly simple, singly trilobate, and composed only of a cellular tissue, which is more fully organized at the summit than at the base. He observes that they then become denticulated and laciniated; and perceiving the rudiments of a second petal appear at the base, he arrives at the conclusions that each petal of the resedæ is composed of two opposed and soldered or connected petals; or rather that the corolla of the great part of the resedæ is composed of two verticilli immediately surrounding the pistil. It has been said by many authors, that the centre of the flowers of the resedæ contains a support, surmounted by a lateral discus, the stamina, and the ovary; but this is not the case. In the greater number of species, the support is hollowed at the summit, and forms a kind of calix, the top of which incloses the base of the ovary. The calix is formed of two verticilli, closely attached one over the other. The exterior verticillus is composed of nectarian scales, attached together, equal in number to those of the petals, and alternating with them, while the interior verticillus is formed of the fixed base of the stamina, really monadelphic. Sometimes the edges of all the nectarian scales are developed; but in general all but one become abortions; and in every case the alternation is preserved. The *reseda luteola* is, however, apparently an exception to this rule, as the edge of the only scale which is developed is in opposition to one of the petals. But as it is shown that the petal is composed of two petals fastened together, the alternation, in fact, still exists. From this observation, M. St. Hilaire concludes that, in the flower which is the prototype of the resedæ, the additions take place in the upper, and the suppressions in the lower part. He then proceeds to consider the staminal verticillus, first alone, and next in its relations with the petals. He shows that the movements which take place in the stamina of the resedæ do not result from the ordinary physical laws, but from a vital force which escapes our means of observation, and points out several analogous phenomena in other plants, particularly the *darilla rugosa*, in which the great divisions of the calyx close over the young pericarpium, allow it to ripen like the seed in a pod, open to allow its escape at maturity, and then close again. By an examination of the *reseda alba*, he proves that the number 10 is the type of the staminal verticilli of the resedæ, and that this number, which presents, by turns, alternations and oppositions, is disguised in the different species by multiplications and unlinings

(*dedoublement*). From these examinations it results that the fl of the resedæ is composed—1st, of the verticillus of the ca 2nd, of a verticillus of petals, alternating with the calyx; 3r a second row of petals, opposed to the first, and fasten it; 4th, of a verticillus of nectarian scales, alternating with double row of petals; 5th, of the stamina; and 6th, of the g ceum. Now the flower which is the prototype of the dicotyle presents—1st, a calyx; 2nd, a corolla, composed of as many alternating with those of the calyx, as there are divisions in the la 3rd, stamina opposed to, and corresponding in number wit petals; 4th, the nectary; and 5th, the gyneceum. We the find in the flower of the resedæ as many orders of parts as the in the prototype or pattern-flower. And if, in order to arrive exact comparison, we regard these different orders of parts acco to the rank which they occupy in the floral receptacle, we shal that the second row of petals in the resedæ corresponds wit opposed stamina of the pattern flower, the nectarian scales wi alternating stamina, and the staminal verticillus of the reseda the nectary of the pattern-flower; thus proving that the ex mobility of vegetable organization admits of different parts flower changing their places in kindred genera.

Plants of Chili.—On the same day Messrs. H. Cassin Mirbel made the following report on a memoir by Adrien de J entitled 'Observations sur quelques plantes du Chili.' The m consists of detached observations, which it is sufficient to notice they present any feature of peculiar novelty. The genus *Fr* placed by M. de Candolle near the Rosaceæ, is placed by Jussieu, in accordance with the opinion of M. Don, nearer saxifragæ. M. de Jussieu proposes to unite the genus *Franco* the genus *Tetilla*, in a small group bordering on, but suffi distinct from the saxifragæ, and to be called *Francoacia*. I proposes a new genus, to be called *Ercilla*, and to belong family of the menispermæ. M. de Jussieu gives a more co description of the genus *Villaresia* than had been given authors Ruis and Pavon; and without going the length of as that he has resolved the problem of its proper classification, he out several striking points of analogy between the genus in q and the holly (*ilex æquifolium*). The genus *Decostea*, pla M. Knutt in the suite of the Juglandes, is, from its striki semblance to the *Aucuba*, attributed by M. de Jussieu, witho tation, to the family of the Corni.

A very small plant of the order of the *Onagianes*, discove M. Gay de Draguignan in the mountains of the province of S is made the foundation of a new genus, called by M. de *Gayophytum*, in honour of the discoverer: its principal pec is, that the number of cells (*loges*) of the ovary is only half of the petals.

Among the Euphorbiæ (*Euphorbiacées*) of Chili, M. de Jussieu remarks particularly the *Croton lanceolatum* of Cavanillas, in which he finds the type of a new genus, to which he has given the name of *Chiropetalum*, expressive of the peculiarity of its petals being divided into palmated straps. This genus, and those analogous to it, form a very natural little group in the tribe, remarkable for the colouring principle which is peculiar to them. The author also adds a new species to the genus *Colliguaya*, but considers it as scarcely varying from the *Excæcaria*. He then describes a new kindred genus, which he calls *Adenopatris*, and thence takes occasion to offer some remarks on the groups of *Hippomaniæ* and *Euphorbiæ*, which he proposes to unite into one, in which case this section, or tribe, of the order of the *Euphorbiæ* (*Euphorbiacées*) would be distinguishable by the almost constant existence of a milky sap (*suc laiteux*), and by the extreme simplicity of the organs of fructification, which renders it necessary to have recourse to the original disposition (*inflorescence*) in order to find the generic characters. The memoir terminates with a list of the genera now admitted by the author in this tribe, which is too long for insertion, particularly as we have mentioned all the new ones suggested by M. de Jussieu. The report concluded by recommending the memoir to the approbation of the Academy.

Ligneous Fibres.—On the 19th September, Messrs. Mirbel, Desfontaines and Cassini, made the following report on a work by M. Paiteau, entitled 'Mémoires tendant à faire admettre au nombre des vérités démontrées la théorie de La Hire, touchant l'origine et la direction des fibres ligneux dans les végétaux.' In the memoirs of the Academy for the year 1708, La Hire maintained that the ligneous layers which occasion the increase of bulk in trees have a direction from top to bottom; that they proceed from the buds, of which they are the roots; and that, like other roots, they have a tendency to bury themselves in the earth. In fact the annual beds of the trunk have one end attached to the branches springing from the buds, and the other to the roots concealed in the earth, forming between these parts a necessary connecting link which cannot be broken without injuring both the buds and the roots. La Hire's idea, however, went much further; he imagined that each bud produces roots, which form with the roots of the other buds a sort of case between the old wood and the bark: this case, which is merely the annual bed, descends gradually from the top of the highest trees down to below the surface of the earth, where all the roots separate, and assume the form in which we find them. When this hypothesis is admitted, it is easy to explain how a swelling is formed at the upper part from a wound or ligature on the trunk of a tree; the roots of the buds, not being able to pass beyond that spot, collect into a mass, and strengthen and thicken instead of elongating. Then one of two things happen—either the swelling becomes sufficiently deve-

loped to admit of the communication between the upper and l
part of the trunk being sufficiently re-established, or the dev
ment is insufficient for that purpose: in the first case, the bud
saved, because their roots attain the earth; in the second, the
exposed to die of inanition, and the tree will perish with the
they have no root. This hypothesis of La Hire excited so littl
tention, that when the same thing was asserted nearly a ce
afterwards by Du Petit Thouars, every one (including probabl
learned naturalist himself) believed it an entirely new theory.
twenty-five years Du Petit Thouars defended it with a persever
worthy of a better cause, but he gained no proselytes, as the ex
ments made by Duhamel and others demonstrated the fallacy c
reasoning; but a new supporter of it having come forward i
person of M. Poiteau, the committee nominated by the Aca
have made the following fresh experiments:—If a large ring of
be taken from the trunk of a sycamore maple, and replaced
similar ring from the bark of a red maple entirely devoid of
the latter will graft itself as a cutting or graft would do, and l
it a bed of red maple wood will speedily be developed. The
of the wood will leave no doubt as to its nature. This lig
production cannot be derived from the buds of the red m
because the ring of bark was devoid of any; nor can it result
those of the sycamore, because they could only produce syca
wood. It must, therefore, owe its origin to some other cause
the lengthening of the roots of the buds: and even if the b
wood formed in the trunk of the sycamore below the ring o
maple bark were to be sycamore, and not red maple wood, st
this bed of wood would be separated from the corresponding b
the upper part of the trunk by the whole width of the ring o
maple bark, it is impossible to imagine that it could spring fro
buds of the sycamore. Again, if a large ring of bark be taken
the trunk of a vigorous elm, or one of many other dicotyled
trees, without being replaced by any thing, new beds of wood w
formed in the lower as well as in the upper part of the trunk,
no ligneous production will appear on the ring of wood left ex
by the removal of the bark; the formation, therefore, of the bu
the lower part of the trunk cannot be attributed to the develop
of the supposed roots of the buds, as they could not descend
the top of the tree to the earth through the exposed ring of
without being perceived. The present memoir contains no
facts, but merely gives the theory the powerful support of M.
teau's opinion; the reporters, therefore, conclude, that, as the t
of La Hire is in direct contradiction to facts, the Academy ca
bestow its approbation on the memoir of M. Poiteau.

Generation of Plants.—At the same meeting, Messrs. Sylv
Mirbel and Cassini presented the following report on a memo
M. Giroux de Busaringues, entitled 'Sur la Génération des Plan

sur les Rapports des Sexes dans le Règne Végétal.' The work is divided into two parts; the first of which contains the details of experiments made by the author on hemp, spinach, the *Lychoris dioica*, and wild sorrel, with a view of investigating a variety of phenomena in the generation of plants. M. Giroux has, with incredible patience, made his observations on 20,000 individual plants, 14,000 of which were hemp. In the hemp and spinach, he collected separately the seeds of the top, the middle, and the bottom of the ear, as well as those of the lowest branches and the thinnest stocks; in the *Lychoris dioica* those of the top and bottom of the *trophosperma;* in the sorrel those of the top and bottom of the ear: he then weighed 100 grains of hemp, taken at random from each of the three parts of the last, and found that, assuming the volume of the grains to be in proportion to their weight, those of the bottom part of the ear are the smallest, and those of the middle the largest. He then sowed, separately, each of the qualities of seed, according to the divisions above-mentioned. A short time after the germination, he reduced the number of the hemp to 5000, by suppressing many of the females, which would have injured the development of the others, and by the total removal of the males. Notwithstanding the absence of the latter, all the females which were preserved afforded an ample crop of fecund seed; but it cannot be concluded from this fact, that the fecundation took place without the intervention of male organs, because it is well known, that beneath the envelope of the female flowers of hemp, there are frequently stamina which cannot be removed without mutilating the adjacent organs; and although these stamina are generally malformed, there is nothing to prove that some among them may not enjoy the property of fecundation. A similar experiment, tried on the *Lychoris dioica*, with a similar result, is more conclusive, if M. Giroux be certain that every precaution of isolation was observed, and that all the flowers were either entirely devoid of stamina, or had their antheræ completely removed before the emission of pollen; but he must assure us of this unequivocally before we can admit the conclusion. The following are the general results obtained by M. Giroux from the above and other experiments:—1. The seeds taken from the summit, either of the ear or the *trophosperma,* have constantly produced more females than those taken from the lower part. 2. In hemp, those taken from the lower part produced more females than those taken from the middle. 3. The seeds taken from the thinnest stalks, both from hemp and spinach, produced more males. 4. The hemp seed of medium size produced more females. 5. In hemp, the size of the plants has been in proportion to that of the seeds. 6. The numerical relation between the male and female hemp, produced from seeds developed, some in presence of the males and others in their absence, did not offer any susceptible difference. In the second part, M. Giroux has extended, to a greater length than has ever been done by any preceding naturalist, his observations on the difference between the males and females of a variety

of *monœci*, *polygamiæ*, and *diœci*, not only in their f their leaves, stems, branches, roots, &c. It would be usel their minute differences of organization, as they can onl stood by a close inspection of the natural flower. M. C gined that there are special relations between the male the peripheric beds of the stem, and between the female the central beds; and with a view of ascertaining the generality of the fact, he extended his observations to a number of plants, both endogenous and exogenous. Fro servations he concludes, that when the vegetation of the beds exhausts itself in forming leaves, the plant becomes the production of female flowers; and that when the c exhaust themselves in producing female flowers, the pla adapted for the production of male flowers, and, therefo distribution of the sexes in the monœci and polygamiæ dep relative state of these two orders of vegetation. The rep sider this last theory as, at least, premature, but, on recommend the memoir to the approbation of the Academ

Exogenous Plants.—On the 19th of September, a report by M. Auguste de St. Hilaire, in the name of himself a Desfontaines and Mirbel, on M. Giroux de Busaringues' me evolution and growth of exogenous plants. The report c by giving the following seventeen propositions, as the re arguments and experiments detailed in the memoir. 1. divided into beds (*couches*), more or less exterior, and m interior. 2. There are two causes of vegetation: the gas in the atmosphere, and the humidity existing in the soil exterior beds, or layers (*couches*) of plants are, during th tion, principally subjected to the influence of the atmos interior beds to that of the humidity of the soil. The de of both is in proportion to the relative predominance of influences. 4. At the point of junction of each of the the plant at its birth, these two causes of vegetation com by that combination, produce an interior longitudinal f embraces all the layers of the stem from the pith to th ference, and it is from this fold that the buds derive the 5. Each foliaceous organ of the bud is specially derived of the folds of the superficial beds of the stem. 6. itself produces the bark and a part of the interior be interior parts form the nerves, which are duplicatures united under a cortical envelope. 7. The size of a nerve in proportion to the number and volume of those which it; the size of the petioles to the number and volume of t pal nerves; the size of the stem to the number and volun petioles which it bears; the size of a principal stem to the nu volume of its vessels. 8. When no obstacle exists, every dinal fold formed in the stem continues to the root, a

fold formed on the root is prolonged to the branches. 9. By this continuation from top to bottom, and from bottom to top, the folds determine the prolongation of the cellular tissue, whence afterwards result the vessels. 10. The evolution of a bud takes place in the same manner as that of a bulbous root, conformably to the order in which the leaves composing it are set on or jointed; the lowest leaves of the flower were the outermost in the bud, and the highest the most central. If, then, the stem of an exogenous annual be in imagination traced back to the plate or species of cone, which would be represented by that of the root, the innermost layers would answer to the highest *verticilli*, and the most superficial beds to the lowest *verticilli*. Although this proposition is not in accordance with the observation of former authors, the reporters consider it as highly interesting, and worthy of being carefully examined by physiologists. 11. The word *verticillus* must not be wholly considered in the sense in which it has been hitherto used by botanists; every association of lateral productions of the same nature may be considered as a verticillus, when those productions, if arranged in imagination in a single plane perpendicular to the axis of the stem would move round it without meeting. 12. If the number of branches of the stem of an annual, not taking into account the small upper branches, be divided by the number of the *verticilli*, the quotient will be the number of layers of the lower part of the stem. This interesting observation has been verified by the reporters upon several pieces of *atriplex patula*. 13. The only difference between an annual and a perennial plant is in the duration. The evolution of each bud takes place in the second year, in the same manner as that of the embryo took place in the preceding year. In the branches, as in the stem, the outermost beds correspond with the lowest *verticilli*, and the innermost with the highest. On the other hand, the innermost beds of the branch correspond with the most central zones of the stem, and the outer beds of the former with the superficial zones of the latter. 14. The outer edge of the sap (*aubier*) is always the same; the increase in size of each bed takes place in the interior by the intercalation of new fibres. Thus the development of the *exogenous*, or *dicotyledon*, is in each bed really analogous to that of the *endogenous* or *monocotyledon*. 15. The inner beds press the exterior ones outwards; the fibres of the inner beds become intercalated with those of the outer ones; and thus the plant augments in circumference. 16. In the bark, the most recent fibrous bundles have a tendency to intermingle also with those of anterior formation; they push them outwards and pass beyond them, on the side turned towards the centre; whence it follows that the inner surface of the bark is incessantly changing. 17. The increase in size does not result from the addition of a new body to a body already in existence, but from a centrifugal evolution of the latter; this evolution is operated upwards, along the length of the stem, by the influence of the roots;

and downwards by the influence of the leaves. It will that this theory of M. Giroux differs from that of all for logists, although it has a very slight analogy to that M. du Petit-Thouars. The memoir in which the theory i although long, is not sufficiently so for the purpose, as e tion might have furnished matter for a separate memoir, by such facts and multiplied experiments, and illustra plates as would be necessary to establish its truth. S would have been an entire treatise of vegetable anatomy and would occupy more time than M. Giroux de Busarin main attention is devoted to agriculture, and with whom p studies are but an occasional recreation) could afford to it; and he has, consequently, rather detailed than proved This memoir must, therefore, be considered rather a materials for the exercise of the industry of other physiol as establishing any new theory. In this point of view t consider the work to merit the approbation of the Acaden

African Plants.—On the 26th of September, M. Aug Hilaire read a report on the memoir by M. Vallot, entit sur plusieurs Végétaux mentionnés par les Voyageurs m ont parcouru l'Afrique Centrale.' The object of this m refer to their proper places in the scientific nomenclatu ferent plants mentioned under their vulgar names by vari travellers in Central Africa. The utility of a work of t tion is self-evident; the gigantic strides made by the botany during the last half century, while they have le intimate acquaintance with the minutest properties of p also been greatly instrumental in confining botanists to style of writing, which renders their accounts unintelligi mass of readers. At the same time the descriptions gi scientific travellers, although more picturesque, and there generally interesting, are necessarily deficient in those which render the discovery of importance to the scientific work, therefore, which, by showing the relation betwee descriptions, enables the reader of the work of amuseme to refer to the work of science, must be of immense a and M. Vallot, in the execution of his task, has proved be possessed both of sagacity and information. M. de S however, points out a few errors into which he has ina fallen. Thus the *Ochrademus* is a reseda, and has nothin mon with the *Sodaba decidua* of Forskal. The white-barke *bium* of Senegal has been already described by M. Jussieu, under the name of *Anthostema*, and it was, the necessary to form it into a new genus. M. Vallot is also mistaken in supposing the *Cauza* of Caillé (which he is rig sidering as a *Spondia*) to be the *Monbin* of America; as M. who has travelled both in America and in Senegal, foun

Spondias in the latter country, but not the true *Monbin*. With the exception of these trifling errors, the work of M. Vallot is highly useful; and in recommending it to the approbation of the Academy, M. de St. Hilaire expressed his hope that the author would extend his researches in a similar manner to the plants of other recently-explored parts of the world.

Vegetation in Brazil.—On the 18th of July, M. Auguste St. Hilaire read a memoir containing a variety of interesting particulars respecting the primitive vegetation of the province of Minas Geraes, in Brazil. The primitive vegetation, which has entirely disappeared in Europe, still exists in a great part of Brazilian America, and in the province in question it is particularly remarkable. The whole country is divided into *matos* (woods), and *campos* (open country). The woods belong partly to the primitive vegetation, and partly to human industry. The latter has been exerted to replace the forests which have been burnt; the former consists of virgin forests, properly so called—the *catingas*, which lose their leaves every year, and the *carrascos*, a species of dwarf forests, the trees of which are only from three to five feet high. The province is divided throughout its whole length by a chain of mountains, which extends from south to north, and gives birth to a multitude of flowers. The western part is merely undulated, but the eastern is mountainous; the former is open, the latter wooded; and these two vegetable regions form two zoological regions almost equally distinct. The various shades of difference existing in these two principal regions are included within limits almost as exactly defined as the principal regions themselves; and when, starting from the sea, and commencing from about fifteen degrees south latitude, we direct our course towards the south-west, we traverse in succession the virgin forests, the *catingas*, the *carrascos*, and the *campos*, a sort of vegetable ladder, in which the plants gradually diminish in height, because the humidity of the soil and of the atmosphere experiences also a gradual diminution. As the zone of the forests is divided into several sub-regions, so also even that of the *campos*, or open country, presents two very distinct subdivisions; for in the southern part of the province the *campos* are composed only of herbs and underwood, while in the northern part tortuous and stunted trees are scattered at intervals among the pasture land. If the physical constitution of the province of Minas Geraes has exercised a great influence on the primitive vegetation, its effect has not been less on that which has resulted from the labours of man, and which may be called artificial. Thus in the forest regions, a fetid grass, called 'seed herb' (*herbe à la graine*), takes possession of all the ground formerly covered with trees; but this grass does not show itself in the *campos* at all, and in the northern region its existence is confined to the subdivision of the *carrascos*. The pasture lands formed by this grass are called, in the country, artificial; but the *campos*, which are called natural, must also have been modified

by the presence of man. Every year these natural past set on fire to procure the cattle a fresher grass, and it is a number of annual species must have been destroyed in tation by these repeated conflagrations. A single bur sufficient to modify, in a most singular manner, the pl existing. Scarcely has the grass of a *campo naturel* bee before dwarf plants are seen to appear here and ther ashes; these plants are merely abortions of species nat larger, and intended to blossom at a different period The most trifling labours of man have an effect on the and in some deserts even the halting-place of the travell by the appearance of particular plants. The nature o ficial bed of the soil, no doubt, influences the details of v these provinces, but that cannot occasion the existence the east of the great chain, and of pasture lands on th the forest regions, the hills are very high, and are te ridges; deep and narrow valleys separate these l shade each other reciprocally; the effect of the wind i this country, and the numerous brooks by which it is wat bute to develop its vegetation. When, on the contrary, is only undulated, and there is nothing to impede the cc winds, when the earth is not refreshed by brooks, it is that the vegetation can be vigorous, however good the naturally.

Goëthe a Botanist.—Among the works presented to demy on the 25th of July, was an Essay on the An Metamorphoses of Plants, by Goëthe. In handing this Secretary, M. Géoffroy St. Hilaire stated that he was in the author to present to the Academy the only copy in which, in order to show his respect and esteem for the (Goëthe) had caused a French translation to be anne name of Goëthe, so eminent in the world of fiction, is known to the public as connected with scientific resea M. St. Hilaire therefore deemed it expedient to accompan with a few observations on its nature and contents. O the volume consists of a reprint of the aphorisms publis author in 1790, under the title of 'Essai sur la Métamo Plantes.' This work was disregarded at the time, an was censured for having published it; but his only fault he outstripped the age in which he lived, and publish on plants half a century before there were any botanists read or understand it. The second part of the work is co additions to, and comments on, the first part; and the au takes occasion to vindicate his claim to a place in the world, by proving that great part of his existence has bee with passion and energy to the study of nature; and that fore, is not to be considered merely as a philosopher wholly

in depicting the internal history of man, or as a poet absorbed by the fictions of scenic illusion. After 1810, the philosophic ideas of Goëthe were universally received; and amongst others, the illustrious De Candolle developed them in his 'Principes de la Symmétrie et de la Métamorphose des Plantes.' Goëthe removes the astonishment we might feel at a poet, whose natural dispositions are generally supposed to be adapted only for the appreciation of moral phenomena, having been able to discern with so much precision the laws of the development of the organs of plants, by detailing, in a minute and interesting manner, the history and progress of his scientific studies. In the third and last part, the author examines with much acuteness the various ideas which have been published since the appearance of his work upon the analogy of the parts of vegetables; his peculiar susceptibility relative to the French doctrines is here unreservedly displayed, both in his exultation at the overthrow of what he characterizes as a dictatorship, which had existed too long, and his regret that some of his favourite ideas are not sufficiently encouraged. M. Géoffroy concluded by remarking, that this work had been sent in acknowledgment of an article written by him in the 'Annales des Sciences Naturelles,' entitled 'Sur les Ecrits de Goëthe, lui donnant des droits au titre de Savant Naturaliste.'

CHEMISTRY.

On the Connexion between Chemical and Electrical Action.—On the 11th of July, M. Becquerel read a memoir forming a continuation of his attempts to trace the connexion between chemical and electric action. The two subjects examined in this memoir are the development of electricity by friction and phosphorescence. It has been generally supposed that friction is produced by the reciprocal interlacing of the rough parts of the surfaces brought into contact, but from various experiments there is reason to believe that molecular attraction is one of the causes of friction. This reaction of the molecules on each other, by producing a derangement of their equilibrium, must also disturb that of the electric forces, for it is now unquestionable that electricity is disengaged whenever the molecules of a body are displaced in any manner. Added to which, since chemical action is one of the principal causes of disengagement, we should examine how far the transitory alterations which the surfaces of bodies undergo during friction, exercise an influence on the production of the phenomena observed. And may not these phenomena, which bear so strong a relation to those of heat, be owing, like them, to vibratory motions of a particular species of that ethereal substance which is supposed to be dispersed throughout all space? After noticing some of the general effects of friction, M. Becquerel proceeds to examine them in detail, as manifested in different substances. When two plates of different metals are placed one at each end of a copper wire, and brought into friction with each other, each of them acquires an excess of contrary electricity, whence a current is imme-

diately produced. By trying the different metals in su
this manner, a table is formed, in whieh each metal is
regards those which follow it. This order is precisely t
that which is obtained for thermo-electric effects, in the cl
composed of two of the same metals, when the temperatu
the junctions is raised. It is demonstrated that the kind c
acquired by each plate is independent of the greater or
of friction which each of them undergoes; and, conseq
the effect cannot be produced by the greater or less deg
produced by the friction upon each of the surfaces.
electric effects of friction are similar to those obtained
the temperature of one of the points of junction of tw
would appear that the heat produced by the friction is t
the phenomenon; while, on the other hand, it may be a
not the friction, by augmenting the power of attraction
bodies, heighten the electric effects which result from
of this power, or does it not produce a particular con
the molecules of each body, the difference of which prod
effects? But if two plates be struck against each other w
tion, no electric effect is produced; the two disengaged e
recombine upon the surface of contact, and there is no cur
in this experiment there is a much greater concussion of
cules of the two surfaces, and a much greater liberation of
in the slight friction of the plates against each other.
therefore, admit that if the displacement of the molecules d
simultaneously heat and electricity, these two effects are in
of each other; it is even probable that the body which is
heated, or the parts of which are the most displaced, is t
assumes the negative electricity. M. Becquerel then den
that there ought not to be any electricity rendered free by
tion of metallic cords, although there are electrical phenom
molecule to molecule; and afterwards proceeds to exa
modifications produced in the phenomena by the reduction
the metals into filings, and by variations made in the tem
When the filings of a metal are thrown upon a plate of the sa
the latter acquires an excess of positive electricity, and the
excess of negative electricity. Generally, filings of meta
tendency to negative electricity; but the filings of a positi
will, notwithstanding this tendency, be positive with relatio
tals of a more negative tendency. The author proves, by
of experiments, that these effects are produced, not by a d
in the action exercised on the metals by the air and water c
in them, nor by the heat which is disengaged, but by a diffe
the mode of aggregation of the molecules of the surfaces, an
quently of their faculties of vibration.

The influence of the molecular state on this phenomenon
still more strongly illustrated by means of an apparatus g
rapid rotatory movement to the plate on which the filings are

Thus, either the peroxide of manganese, silver, or sulphate of iron, filed into very small particles and thrown on to a plate of zinc, tin, or gold in motion, assumes a negative electricity. Filings of zinc when thrown upon a plate of the same metal in motion exhibit no electricity, but become electric when the plate is in repose. Hence it appears that the swiftness of rotation augments the negative tendency of the zinc plate, probably by producing a concussion of all the molecules on the surface. There is great reason to suppose that the electric effects of decomposition and recomposition, operated during the concussion of the molecules, may furnish a clue to the cause of the magnetic phenomena observed by M. Arago in metallic plates when in motion. The general result of all M. Becquerel's experiments establishes that, when friction is produced between any two metals whatever, either in repose or in motion, that metal the parts of whose surface are most displaced becomes affected with negative electricity.

M. Becquerel also examines the effect of friction in bodies which are bad conductors of electricity; and although the variation of these effects, according to the state of the surfaces, renders the solution of the problem more difficult than when metals are employed, the relation between the phenomena in the two classes is plainly to be perceived. Thus fibrous substances are more strongly susceptible of electric affection than other bodies, because their particles are more easily displaced; for the same reason, heat augments the negative tendency of bodies. From these experiments M. Becquerel was easily led to consider the phenomenon of phosphorescence, and a great number of experiments have convinced him that electricity is disengaged whenever a change of equilibrium is operated in the molecules of bodies. This phenomenon he considers to consist in the separation of the two electricities, the composition of which produces, according to its greater or less rapidity, light, heat, chemical or magnetic effects.

Phosphorescence is produced by heat, light, percussion, the electric shock, certain chemical actions, and sometimes by the exposure to a high temperature, which occasions the body to lose its faculty of entering into combination with others. M. Becquerel proves that all these causes may disturb the equilibrium of the electric forces without occasioning free electricity to be disengaged. He has also proved that, when two bodies combine, that which acts the part of acid assumes, with respect to the other, a positive electricity, and that which acts the part of alkali assumes a negative electricity. In many cases these two electricities instantly recombine; but when the action is slow and the bodies are bad conductors, recomposition does not take place until the two electricities have both acquired a tension sufficient to enable them to overcome the obstacle opposed to their reunion by the want of conductibility. This is probably the cause of the phosphorescence produced in some slow chemical actions which take place spontaneously in the air, as in the earthy

sulphates, rotten wood or fish, &c. There are numerou in which phosphorescence may be explained by the reco the two electricities disengaged in consequence of a der the molecules of bodies. On cleaving a crystal light i observed, which is evidently electric. But may we no bute the phosphorescence produced by heat to a parti or a displacement of the superposed plates? If cer after having been highly heated, lose the property o phosphorescent, is it not because the heat has produced tive situation of their molecules a derangement which re regular displacements impossible? And is not the lu pearance produced by percussion, and even that pr grinding, when no free electricity is disengaged, to b to another change in the position of the molecules, and c to an electric decomposition and recomposition? By a si cation of the same principle, M. Becquerel accounts fo phorescence observed in some bases, such as zircon, whi is produced, deprives those bases of their faculty of com the acids. Finally, M. Becquerel considers that the phos produced by electric discharges, which is observed in b are bad conductors, depends on a species of cleavage w rise to a decomposition and recomposition of electricity. is only successive on account of the bad conductibility of and during the whole time that a portion of the two which have become free remains engaged between the m a body, that body has a luminous appearance.

Bromide of Silicium: New Chemical Compound. 26th of September, M. Serullas read a note on a new co bromine and silicium, which he calls bromide of silicium. pound is obtained by mixing lamp-black, pulverized su sufficient quantity of oil to form a homogeneous paste, w hydrated and desiccated to a certain point. This paste m cined in a covered crucible. The quantity of carbon contai different substances employed should be above half the wei silica. The carbonaceous and spongy residue of the cal introduced in small fragments into a porcelain tube, at o extremities of which is fixed a small retort containing the and at the other a tube, which terminates in a globe or surrounded with ice, and having affixed to it a long tube, te by a narrow opening. The porcelain tube being made inca the bromine is volatilised by slow degrees by means of l bromide of silicium is produced and condensed in a liquid fo tube and receiver. When the operation is terminated, the should be re-distilled, in the manner pointed out by M. for the chloride of the same base, after having shaken it same retort in which it is to be distilled, with mercury, in get rid of the excess of bromine; this produces a *magma* o

or less thickness, which appears scarcely to contain any liquid, although a considerable quantity may be obtained from it by distillation. Bromide of silicium, when distilled, is nearly colourless; it emits thick vapours in the air. When cooled in a frigorific composition, it becomes solid at from 12 to 15 degrees below zero (— 10° or — 5° F.); partaking, in this respect, of the property of bromine. It is raised to a state of ebullition at from 148° to 190° (300°—374° F.); its density is greater than that of sulphuric acid, for it falls rapidly through that liquid, in which it is decomposed but slowly, it not being until after the lapse of several days that it is entirely converted into silica and bromine; the latter being the result of the reaction of the sulphuric acid on the hydrobromic acid. Potassium acts violently on bromide of silicium at a very slight elevation of temperature, producing a loud detonation which constantly fractures the tube. The following are the principal points of difference between the chloride and the bromide of silicium. 1. The chloride boils at 50° C., the bromide not until 150°. 2. The chloride, which sinks in water, has less density than sulphuric acid; it remains on the surface, and is there decomposed into silica and hydrochloric acid. The bromide, on the contrary, is, as we have before seen, heavier than sulphuric acid. 3. Potassium suffers no sensible alteration from chloride of silicium in a state of ebullition, whereas a very slight heat is sufficient to produce a violent action of that metal on the bromide; which may be accounted for thus: The potassium becomes fused before the ebullition of the bromide; the chloride, on the contrary, boils before the fusion of the potassium can take place. Indeed, if potassium which, from having been exposed to the air, is beginning to liquefy, be dropped into bromide of silicium, the detonation will take place instantly. 4. The chloride of silicium may be cooled below — 20° C., without losing its liquid form; whereas we have seen that the bromide becomes solid at from 12° to 15° below zero. The bromide of magnesium may be obtained in the same manner, by a mixture of carbon and carbonate of magnesia, &c.; but it is difficult to procure it pure, because it is not volatile, and does not fuse under a red heat; then, as fast as it is formed, a part is carried off by the gas into the tube and the ball, which are obscured by it under the form of a greyish powder, a mixture of chloride of magnesium, magnesia, and carbon; and another part remains at the bottom of the porcelain tube, and in the first part of the tube which corresponds with it, under the form of a molten mass, more or less pure, whitish, and crystalline. Bromide of magnesium has a powerful attraction for the humidity of the atmosphere, and acts strongly on water, producing detonation and development of heat.

Geology.

Pyrenean Chain.—On the 26th of September, M. Neboul read an interesting series of ‘Observations on the Structure of the Pyrenees,’

of which the following is an abstract. He stated, that in end ing to determine the direction of the Pyrenean axis, and its both with the direction of the inclined strata, and with the p parts of which the entire chain is composed, he arrived at lowing conclusions:—1. That the Pyrenees are not directe E. S. E. to W. N. W., but at least fifteen degrees southward line. 2. That the direction of the strata is rarely parallel axis. 3. That the Pyrenees do not constitute a simple chai may be supposed to have been formed at a single ejection. they exhibit the traces of various subterraneous evulsions, b they may be supposed to have been produced. 5. That the sions, which appear to have succeeded each other during t durations of the ancient periods, were, like those of the Al tinued into a considerably advanced epoch of the tertiary Both Pliny and Ptolemy have fallen into an error, in fixing the promontory of the Pyrenees at a spot called Aso, which I supposes to be the *Punta de Figuerra*, near the mouth of the I and Gmelin the Cape Machicaco; but neither of these poi the termination of the Pyrenean chain. This chain, to w promontories in question are mere appendages, leaves the north, and extends to the confines of Galicia, as was obse Strabo. This error has been very universally adopted, an the direction of the Pyrenean chain has been usually state from Cape Creus to the Punta de Figuerra, two extreme poi of which is situated south, and the other north of the true of the Pyrenean axis. This axis really commences in the Cape Cerveres, the crest of which forms the best separation the torrents directed towards the north, and those directed the south. Its western termination is more difficult to de certainty, because, on approaching the sea of Galicia, the ch out into two branches, one of which terminates at Cape Ort the other at Cape Finisterre; a line drawn from Cape Ce the point where the separation takes place, and thence ext the sea, would terminate between the two capes near Coru the island of Sisarga. This direction, which alone fulfils ditions prescribed for a geographic axis, differs only six degrees from the parallel of the equator. It varies but li the extreme sinuosities of the crest or ridge, formed by declivities of the chain, divides the mountainous regi equally between these two declivities, and most naturally u extremities with the centre; the most remarkable summits culminating points, whence proceed the principal fluvial such as the Aude, the Arriège, and the Garonne, in France Ebro, the Douro, and the Minho, in Spain. A chain several geological axes, arising from the direction of speci or other causes; but these axes must be partial, except w are, by parallelism, confounded with the central and geo axis, which is, by its nature, single and universal. A gre

does not appear to exist in the Pyrenees. The masses of granite form, as it were, large islands in the chain, which do not agree either with themselves or with the geographical axis. The western region of the French Pyrenees contains tracts of aphite, having nearly the same direction as the chain from east to west; but they do not exist in the eastern region. The French Pyrenees, particularly those valleys through which flow the streams tributary to the Garonne and the Adour, contain a number of oblique ridges, which, as well as their strata, are directed towards W. N. W., and sometimes even towards the N. W., whence has arisen the great error of applying this law to the whole chain, and supposing the direction of strata to be parallel to the Pyrenean axis; whereas, in fact, that axis and the strata directed towards the W. N. W., cut each other in an angle of at least fifteen degrees. This error might have been avoided by observing that the rule above alluded to is by no means a general one; some of the ridges of the Pyrenees are directed towards the W.S.W., and the strata follow the same direction; that of Canigou, for instance, on the summit of which are seen gneiss and micaceous schistus, having the same direction as the protuberance of which they form the pinnacle. A few strata are also found having the same direction as the total chain from east to west. The sinuosities of the small ridges, their obliquity with respect to the central axis, and their junctions in one sinuous summit, prove that the Pyrenean chain was thrown up at different epochs. This fact, which is derived from the irregularities of detail in the central crest, is confirmed by the relation of its great and principal divisions. Independently of the small chains which may be traced on the two declivities, there are three principal and distinct chains, which contribute to form the long summit of the Pyrenees. The ridge or chain which overhangs the eastern region follows the direction from E. N. E. to W. S. W. It extends from the plain of Roussillon to that of Catalonia, skirting in France the right bank of the Est, and in Spain the left bank of the Segre. Its numerous summits attain heights of 1400 to 1500 toises; the Perigmal of Cerdagna is the most elevated point. The central crest, which is cut by this ridge between Mont Louis and Prati de Mallo, is much inferior to it in height. The valleys of the Est and the Segre form, at the foot of this chain, the only longitudinal section of this nature which exists in the whole Pyrenean chain. The great basin of Cerdagna (the largest in the Pyrenees) occupies the culminating point of the double valley, which appears to be situated in the linear direction of the summit, about 600 toises below the highest point. To the N. W. of this basin rises the second ridge, directed towards the W. ¼ N.; its height is nearly 1500 toises, at the source of the eastern Arriège, and exceeds that elevation in the region of the western Arriège. It extends into the region of the Salat, and to the first branches of the valley of Etran; then gradually diminishes in height, and is lost amid the mountains of the French declivity.

This is the chain, the linear direction of which would, if]
terminate near the mouth of the Bidassoa. The general
the Pyrenees passes suddenly from the ridge, to anot
southerly, which is the principal, as it embraces in its almo
linear direction the most remarkable points of the chain
easily be seen that this ridge is not, as has been suppose
geologists, united with the preceding by means of a win
and that the two ridges only join by their inverse decliv
basin of Beret, which, as well as the great basin of Cerd
formerly a lake, the waters of which must have flowed
ously towards France, and towards Spain. We, therefor
the Pyrenean chain, simple as it is, is composed of seve
having different directions, both in their masses and in th
which proves the error of M. Elie de Beaumont, in sup
Pyrenees to have been formed at a single ejection, by the
of his own principle, that eminences having different directi
result of different evulsions. The Pyrenees contain nume
cations of rocks thrown up at various epochs. The most
these is the presence of dry storax in the grauwaken of
and in the deposits of anthracite of the intermediary f
The period at which these ancient formations were thrown
well be ascertained, but it is certain that when they were f
plants, the remains of which are buried in them, crowned t
round their basins, which heights were already mountains.
spot in which the sedimentary tertiary formation, not all
similar to that of the Herault, and the Apennines, is found
with the Pyrenean rocks, is where the Est empties itsel
plain; for from the borders of the sea of Gascony to the
the Est, in the Mediterranean, the chain appears surro
alluvial formations; but at Nassaich, near Millas, the sa
shelly deposit leave uncovered a large fragment of *mola*
bluish sandy marl fixed to the Pyrenean quartz-rock. T
fact is sufficient to destroy the hypothesis of the Pyre
anterior to the Alps, and would even, perhaps, authorise
conclusion, as the glauconian deposits which, in the Pyren
the central point of Mont Perdu, are only met with in the A
eastern summits and at medium heights; such as the m
Fis, near Serrais, and that of Diableritz in the Lower V
the *molanes* which, in the Pyrenees, rest immediately on
of the central ridge, in the Alps, do not attain that ri
but occupy only a part of the exterior chain, which,
to Saussure, belongs rather to the system of the Jura, than
the Alps of Mont Blanc. In conclusion, M. Nebaul re
as the greater part of mountainous systems (except the vol
have a great resemblance in the composition and the dis
their rocks, it is probable that their differences arise much
the accidents of locality than from any general relations de
the period of the commencement or completion of their

This formation is the most striking and the most universal of the ancient geological periods. The indications of the throwing up of rocks occupy some epochs of the primitive, and all those of the secondary period; they are also frequently remarked during the tertiary period, when the great terrestrial evulsions which had produced the large chains of mountains began to be replaced by the volcanic eruptions and earthquakes, of which we are still witnesses.

Medical Science.

Cure of Burns.—On the 4th of July M. Magnin de Grandmont addressed a letter to the Academy, detailing several cases in confirmation of his theory of immersion in cold water being an infallible remedy in all cases of burns affecting only the *epidermis*, which, he remarks, are by far the most numerous class of burns.

Cholera Morbus.—On the 18th of July M. Magendie read a letter from M. Scipion Pinel, a surgeon at Warsaw, in which it is maintained that the cholera morbus affects principally the sympathetic nervous system, as is proved by the weakness produced in the general circulation, which is not accounted for by any sufficient affection of the heart, or the organs of circulation, and can only, therefore, be produced by a diseased state of the nerves, particularly the grand sympathetic or *trisplanchine* nerve. He therefore proposes to give the disease the name of *trisplanchine.* M. Pinel adds, in proof of the disease not being directly contagious, that he has infused into his own veins, not only the blood of a dying patient, but even the intestine mucus taken from a dead body. But he remarks, that when he remains more than a quarter of an hour in the room with the patients, he experiences a feeling of painful oppression in the stomach, in the direction of the vertebral column, which is removed by going into the open air. The treatment recommended by M. Pinel differs principally from that hitherto adopted, in prohibiting narcotics; but he agrees with other physicians in recommending warm drinks, and all other applications tending to restore heat to the surface of the body, and increase the circulation.

Anatomical Plates.—On the 25th of July, the Academy, on the recommendation of its reporter, M. Dumeril, bestowed its special approbation on the 'Traité Complet de l'Anatomie de l'Homme,' by MM. Bourgery and Jacob. This work is illustrated by 500 plates, executed with the greatest exactness.

Lithotrity.—On the 5th of August M. Civiale communicated to the Academy a case of lithotrity, in which the calculus was six inches in circumference. Great difficulty was experienced in fixing it in the instrument, although the orifice of the pincers was twenty-six lines in diameter. The operation was completed in fourteen visits.

The first ten were employed in perforating, and the last fou ducing the stone into small fragments, which were passed c the urine. No ill effects whatever attended or followed the tion, and the patient is perfectly recovered. The *detritus*, pa with the urine, was exhibited to the Academy, (having been c into small masses by means of a solution of gum,) and excit versal astonishment and admiration. The patient was present meeting.

Cure of Fever.—At the same meeting M. Magendie made favourable report on the use of powder of holly leaves, recomn by Dr. Rousseau as a cure for fever (*vide* page 148). The re stated that the new remedy had been tried in the hospitals in tl different cases of fever. The doses administered were from one *gros* per day, and in every case the patients were cured after twenty days treatment. The effect of the holly is not so qu that of the quinia and silicine, but it is a sure and excellent feb The only thing necessary to make it thoroughly useful, was to e its essential properties, so as to avoid the necessity of adminis it in such large quantities. We have already mentioned tha has been done; and M. Magendie concludes his report by s that ilicine may now take its place with quinia and silicine in tl of febrifuges.

Cholera at Mecca.—On the 12th of September M. D'Arcet municated to the Academy a letter which he had received fro Mimaut, the consul-general of France, in Egypt, containin account of a contagious disease, which had carried off at least 1 pilgrims at Mecca, and was still raging there with great viol The individuals attacked fell down in the street, without any pre illness, and, after violent vomitings, died almost instantly. visitation was at first considered to be the plague; but the I repelled this idea, on account of the promise of the Prophet, pestilence should never visit the Holy City. They preferred a buting it partly to the want of soft water, which had existed for s time in Mecca, and partly to the vengeance of the Deity, at his house having been so long violated by the infidel drums and tr pets of the regiments in garrison at Mecca: the latter cause been removed by the colonel of the regiment imposing silenc his band. There appears, however, every reason to believe that disease is no other than the cholera; and the immense influx of grims from every quarter, including Persia and India, added to intense heat (31 deg. Reaumur), furnish sufficient causes for propagation of the epidemic. During the three days devoted religious ceremonies, previous to the Bairam, the whole body of grims remain agglomerated in a dense mass, and do not move e when the rain descends upon them in torrents and numbers fall d around them. During these three days the mortality was terri

and immediately afterwards, was still more increased by the feast of Mina, at which every Mussulman kills a sheep, the blood and entrails of which are left to rot on the public ways: thirty thousand of these animals were killed in one day, and the putrefaction resulting increased to such a degree the intensity of infection, that Mina was covered with corpses like a field of battle. The governor, Abdenbeg, who would not neglect his religious duties, went to Mina to assist in the sacrifice of sheep, and being attacked with the disease in the night, had ceased to exist before the morning. Annexed to this letter is the *procès verbal* of the *post mortem* examination of two of the corpses by European surgeons at Mecca; the symptoms are similar to those observed in cases of cholera elsewhere. This interesting communication was referred to the Cholera Morbus Committee.

NATURAL PHILOSOPHY.

Polarized Light.—On the 11th July M. Babinet communicated the result of some experiments which he had made relative to the unequal absorption of the two polarized rays of the coloured crystals which have a double refraction. He states that 'all the negative crystals, such as coloured spath, arragonite, tourmaline, &c., allow the ray, which undergoes the extraordinary refraction, to pass in excess. All the positive crystals, on the contrary, such as smoky quartz, the gypsum of Montmartre, &c., transmit the ordinary ray in great abundance.

Vibration of Sound.—On the 18th July M. Savart communicated the result of his experiments made with an instrument, invented by himself, for the purpose of ascertaining the greatest and least number of vibrations per second of which a sound may be composed so as to be perceptible to the human ear. He had previously ascertained that, in one extreme, sounds resulting from more than 40,000 simple oscillations per second, may be distinctly perceived; and he now stated, that, in the other, sounds may be produced by his machine, which are not only perceptible, but even intense, although composed of but eight vibrations per second. The lowest limit of perceptible sounds produced without the aid of his machine was thirty-two vibrations per second.

Conduction of Sound by Water.—On the 8th of August, a letter was read from M. Cagnard Latour, communicating an experiment which he had made with the instrument called the *Syren*. It is well known, that if the instrument be set in motion by a column of water of sufficient elevation, a sound resulting from the vibrations of the liquid itself is produced, even when the instrument is completely submerged. M. Latour ascertained, that by plunging himself into the water, and putting the *syren* in motion by injecting the liquid by

means of a pump held in his hands, the sound increased in the moment his ears were submerged, although his distance instrument remained the same, thus proving that the hydrau tions were directly transmitted to the auricular organs w energy than when transmitted through the medium of tl sphere. M. Latour also found, that the intensity of the s not vary materially in proportion to the depths to which merged himself; whence he concludes, that the augmentati pressure of the air contained in the ears did not operate on nomenon, but that it depended mainly on the immediate l communication.

Oscillations of the Pendulum in Air.—On the 22d of M. Poisson read a memoir on the simultaneous movem pendulum and the ambient air. The learned academici remarking that M. Bessel was right in asserting that so modifications must be made in the great law of nature laid Newton, that all molecules attract each other in the direct their mass, and the inverse ratio of the square of their stated that he had repeated several of the experiments of M He put pendulums composed of substances of unequal weig state of oscillation, and ascertained that the variation in th occasioned the variation in the number of oscillations.] after a great number of experiments, all of which he verifie culation, satisfied himself that the gradual diminution of tl tude of the oscillations of the pendulum must be attribute pressure of the ambient air.

ZOOLOGY.

Silkworm.—On the 4th of July, M. Duméril read a rep memoir by M. Lamare Picquot, relative to the *Bombyx p* Asia. M. Lamare Picquot, while in India in 1829, had foun forests of Bengal which line the right bank of the Damo the west of Calcutta, some silkworm cocoons, the silk of w peared to be of so excellent a quality, that he was induced the insect sought for, and succeeded in procuring severa silkworm lives in the forests on a kind of wild *Badamier* (*minalia* of botanists), which is very common. The India keep these worms, feed them on the leaves of the ordinary *bc* or with those of the *Shamnus jujuba.* The insect undeı last change on the return of spring, and comes out of the co a hole which it makes in the extremity. The female soon af lays her eggs, and both male and female shortly die. T come to maturity in about twenty to twenty-nine days, and o in the month of March. The worm at its birth is about a quart inch long, yellow, with the head black and large. At its full it is from three to four inches long. M. Lamare Picquot d

some particulars from Dr. William Roxburgh, who has given an account of this insect in the seventh volume of the Transactions of the Linnæan Society of London, as to the time of its various metamorphoses. The latter states, that the *Bombyx paphia* constructs its ball in the month of October, and that the perfect insect does not make its appearance until the July in the following year; so that its captivity would last some months. It is, however, certain, that the cocoons brought into France by M. Lamare Picquot, and placed in hothouses, were hatched in the spring. The reporter, however, remarks, that the appellation *paphia* strictly belongs only to the *bombyx* represented by Cramer under that name, plate 147 A and B, and 148 A; the species to which the same name has been applied by Linnæus and Fabricius are so imperfectly described, that it is almost impossible to know what they really are. The *Bombyx paphia* always comes into the world during the night; its wings are entirely developed in two hours—the distance from tip to tip of those of the female is nearly five inches. The number of males generated at one time is but one-fifth of that of the females, so that one male impregnates several females. The desire of reproduction manifests itself almost immediately after the development of the wings, and is shown by a shrill buzzing noise, well known to the Indians, who attach the females to the branch of a tree by a silken thread tied round one of their legs, and in the morning they invariably find them with the males attached to them. The males are active, and take long flights; but the females are heavy, and fly but little. The colour of the female varies considerably, but that of the male is uniformly of a deep brick-red. The copulation lasts from twelve to nineteen hours, and the number of eggs varies from 500 to 700; they are white, and occupy the greater part of the abdominal cavity. The cocoon is of a very singular construction, and different from all those which are known to us. It is not fixed to the branch of a tree by a glutinous matter or by a silken thread, but the insect chooses a branch which is about half an inch in diameter, and forms a species of ring round it with a resinous matter issuing from its mouth; it then extends its work in a sort of pedicule, of about two inches long, in which it gradually encloses itself. The Indians, to preserve it from the birds, and to prevent the females from leaving the spot, cover the tree with a thick net. The silk which comes from these cocoons has a dark tint, which must be chemically removed before it can be dyed any other colour; it is much coarser than the silk of the common silkworm, but is stronger, and the stuffs made from it last a long time. It is also used to make nets and fishing-lines. M. Lamare Picquot imagines, that some of the trees now grown in France may probably be found to answer as a substitute for the *badamier* in furnishing food for these insects, and wishes them naturalized in France; but the reporter remarks, that although it would be easy to have the insects from India, it would be absolutely necessary to be provided with a nourishment for them—as otherwise, should M. Lamare Picquot

not be right in his conjecture as to the fitness of some of t
trees for that purpose, they would necessarily perish. In c
the reporter, though bestowing great praise on M. Lamare
assiduous labour, appears rather to discourage the introduct
Bombyx paphia, on the ground, first, of the uncertainty
able to nourish it; and secondly, that if the silk be useful,
probably be as economical to get it in a raw state from Indi

At the conclusion of this report, M. Chevreuil stated th
been engaged in endeavouring to bleach the silk in question
not yet been able to succeed in doing so; although he h
derably diminished the intensity of the dark colour. He add
is undoubtedly chemically different from common silk, a
require to be carefully analysed before it was adopted as an
commerce, since the chemical tests now used at the Custom
ascertain the purity of common silk would not produce t
effect on this. M. Chevreuil concluded by expressing an
that this silk is not a simple substance, but a combination o

Apoplexy in Horses.—On the 15th of August, M. Bo
sented a memoir to the Academy, from which it appeared
plexy of the spinal marrow is as frequent in horses as apo
the brain is in men; an observation in perfect and curiou
ance with the relative degree of activity of those two orga
two species of animals.

Teeth of the Gnawing Mammiferæ.—On the 11th of J
Géoffroy St. Hilaire read a memoir, the object of which was
that the front teeth of these mammiferæ, which have been
called incisors, are, in fact, analogous to the canine teet
this purpose he entered into the history of the names give
teeth in different animals. In the human anatomy, tee
divided into three classes—the incisors, the canine, and the m
grinders; and the same names were without difficulty applied
families whose organization most resembled that of man, suc
quadrumani and the carnivorous. After these families, h
came animals which are digitated, but have but two sorts o
and this circumstance became the characteristic of one of th
orders of mammiferæ, that of the *rongeurs*. There was no
time spent in inquiring in which class of teeth these anima
deficient. Teeth were observed placed in the front part of the
incisors are in man. The front teeth of the *rongeurs* were, the
immediately called incisors, which name some zoologists afte
changed to *primores*,—a term which had the double advan
expressing that these teeth are the first which offer themse
observation, and that they present a characteristic of the first
of zoological importance, inasmuch as their variation is alway
nected with a great number of others in the organization. The
of incisors was, therefore, given to these front teeth, merely b

that is the name of those teeth which, in the human mouth, present themselves the first in going from front to back. If the inverse course had been adopted, and the calculation made from the hinder part of the jaw, those teeth which, in the *rongeurs*, come after the molares, would, for the same reason, have been called canine. M. St. Hilaire's object is to ascertain which of the two appellations is intrinsically correct. It is evident that one class of teeth is entirely wanting, and this deficiency may be considered as the result of an atrophy. This atrophy must have existed either at the middle of the jaws, or one of the extremities; and M. Géoffroy conceives that the latter hypothesis is alone admissible, and that the atrophy must have occurred at the point at which the maxillary branch terminates. When the length of jaw gives sufficient room for the full development of the dental nerve, as in the dolphin, the lizard, the crocodile, &c., the dental, arterial, venous, and nervous branches are subdivided into clusters, of similar volume, and equally distributed. Then there are as many conic and symmetrically arranged teeth as there are subdivisions in the parent branches. There is then a regular formation in every point, both anteriorly and posteriorly; and it is of little or no consequence to what zoological class the animal belongs, since it is not the organic difference, but the room which exists for the development and distribution of the vascular and nervous branches, which makes the distinction. Hence we may conclude that there is nothing specially inherent in the nature of the dental operations to occasion the division of the teeth into the three classes of incisors, canine, and molares. The want of one class of teeth in the *rongeurs* is, therefore, owing to the want of room for development; the development began with the molares, which are, unquestionably, there; then proceeded with the canine teeth; but being there stopped for want of room, ceased, and the incisors are consequently wanting. There is no reason why we should admit the existence of the two extremes of the molares and the incisors, and suppose the absence of the intermediate class, the canine. The front teeth of the mammiferæ *rongeurs* should, therefore, as a matter of consistency, be called canine, and not incisors.

On the 18th, M. St. Hilaire read another memoir relating to the same subject; the object of which was to correct an error into which, the learned academician stated, that he considered himself to have fallen, in common with M. Cuvier, in 1795; when they published a memoir, in which the existence of the inter-maxillary bone was considered as furnishing a certain criterion for attributing to teeth affixed in it the character of incisors. This opinion M. St. Hilaire now considers erroneous:—1. Because it can only apply to the upper jaw, and, therefore, leaves the teeth of the lower jaw subject to an arbitrary classification; and, 2. because, so far from the inter-maxillary bone being merely intended to support the incisors, that bone exists in a great number of animals which have no teeth at all in the an-

terior part of the mouth. The inter-maxillary bone is pri relation with the organs of taste and smelling; and, like n cranium bones, its principal purpose is to furnish partitions in forming the cavities in which the organs of the senses a It is true that these bones also furnish sockets for the teet is quite a secondary function. The teeth, stony and crystalli stances, have a structure and system of formation which re wholly unconnected with the structure and form of the osse Deposited at first on the maxillary arcades, they do, it is tr out a socket for themselves there; but this intercalation determined by the accident of proximity, and is not produc marked predilection for a particular bone. In those anin have long jaws, the teeth, not meeting with any obstacl development, are regular in form and position all along This could not be the case in man, because the extreme ment of the encephalos rendered a corresponding reducti face necessary. As, however, the nerves and vessels w through the jaws are not less numerous, and each of thes must terminate in a tooth, it follows that the number of t germs is not less considerable, but their arrangement is les In the parts nearest the origin, these bundles are for groups of four, whence result the teeth with four fangs; f they are only two and two, and the teeth have two fangs; v only towards the extremity that the germs are developed in lated manner and produce single teeth. It is only in the miferæ which have the cerebrum large and the face short find those teeth with several fangs, which must be cons being produced by dental germs, heaped, and, as it were, together. These explanations M. St. Hilaire considers as confirming his theory respecting the teeth of the mammife *geurs*, as above developed.

Snail's Eggs.—On the 15th of August, a letter was re M. Turpin, containing some particulars of the microscopic of the egg of the garden snail (*Helix hortensis*). When the of this egg is viewed through a strong magnifying glass, the surface presents an infinity of white points, which appear, as drowned in the soft mucous and transparent envelope of t When an egg is crushed between two plates of glass, all the and albuminous liquid which it contains is scattered abroad, torn membrane remains empty. If the whole be then viewed a microscope having a two-hundred times magnifying power digious quantity of very beautiful pointed white and transluci boidal crystals, regularly formed in their angles and sides, are distinguished. These rhomboids are of unequal dimensions, the being about 100th part of a millimetre (.0004 of an inch). S single, and others grouped together by two, three, four, five, a all are fixed or glued against the interior surface of the enve

the egg. Among these rhomboids are found a few cubes, and some regular prisms with square bases. The number of these crystals is so considerable, that they may be considered as forming at least half the volume of the egg. The white external points above mentioned indicate the crystals which line the interior surface of the envelope. After these crystals have remained six days between glass plates, their form gradually alters, their angles become rounded, their beautiful white changes to a yellow hue, and they are in a great degree liquefied. These crystals were observed by M. Turpin in an egg laid by a snail on his table, and instantly examined with the microscope; whence he considers it probable that they are formed in the egg while it is yet in the ovary of the mother, in the same manner as similar crystallizations are formed in the cellular tissues of plants, particularly those of the genus Cactus. MM. Chevreuil and Cordier were requested to verify these remarks, and also to examine whether these crystallizations are peculiar to the garden snail, or whether they are common to every species of the genus *Helix*, and to the eggs of all the molluscæ or *molocogoanes;* and also whether similar crystallizations could be obtained by submitting the albuminous liquid of the eggs of birds, reptiles, and fishes, to the action of electricity We shall, of course, communicate the result of their investigations.

MISCELLANEOUS.

Human Nutrition.—On the 11th July a letter was read from M. Roulin, a young physician of eminent attainments, both medical and scientific, in which he vindicated the nutritive properties of gelatine, and pointed out the absolute necessity of salt entering into the regimen of animals upon whom the effect of different alimentary substances was to be tried. As a proof of the manner in which animal strength may be supported, he related the remarkable fact, that in travelling through some forests in Columbia, in 1825, he and his guides, being entirely without provisions, were compelled to eat five pair of sandals (made of untanned leather, softened by the dampness of the forests) and a deer-skin apron, which they roasted and masticated. In the latter operation, two hours were occupied in getting through the third part of the sole of a sandal. This singular aliment supported their strength; and though the journey, which was to have lasted only two days, occupied fourteen, they arrived at its termination in good health. They occasionally ate the core of the palm-trees, but found that it sustained their strength much less than a piece of the roasted leather.

New Compressing Pump.—On the 18th July a letter was read from M. Thilorier, announcing that M. Perrot had recently applied his new system of compressing gases to an engine of war, by means of which it throws 200 balls per minute. This machine principally differs from that of Perkins by the use of the elastic force of air instead of that of steam, by which a considerable saving of expense

is obtained. In the ordinary pumps, the force required t the resistance of the piston is as the number of atn whereas in M. Thilorier's new pump, the force required the square root or cube root of the atmospheres, accord gas has been submitted to two or three successive con Hence by the new pump a single man may, in a given tim as much work as a steam-engine of thirty-horse power ap old pump.

Climate of Asia.—On the 18th of July, M. de Humb municated to the Academy some very curious observatio relation subsisting between the temperature of the soil an nomenon of the preservation of the soft parts of antedil mals. The first basis of climatology is the precise knowle inequalities of the surface of a continent. Without this k we should attribute to the elevation of the soil what is, i effect of other causes exercising their influence on the lo (in a surface which has the same inflexion as the surfa ocean) upon the inflexion of the isothermal lines. In from the north-east of Europe to the north of Asia, b forty-sixth or fiftieth degree of latitude, we find at once a in the mean temperature of the year, and a more unequal di of this temperature among the different seasons. Europ sinuous shape, is but a peninsular prolongation of Asia, a (renowned for its mild winters and unoppressive summers rest of France. The predominant winds received by E the west winds, which to the western and central part breezes, that is to say, currents which have been in cont mass of water, the temperature of which, at the surface b and 50 degrees of latitude, is never, even in January, below grade. Europe enjoys the influence of the large terrestria zone of Africa and Arabia, which becomes heated by the diation in a far different manner from that which would b with a surface of water similarly situated, and which, by the ascending currents, pours out masses of hot air on the situated more to the north. The small and unequal develo Europe towards the north, and its oblique direction from sc to north-east, are advantages which have not hitherto b ciently appreciated in considering it with respect to its gen figuration, and as a western prolongation of Asia. Be placed opposite to the gulf which the warm waters of the gu open in the polar ices, its coasts are (at least in the two-thir are western, that is, the part properly peninsular) bathed l sea; for, in the one-third which is eastern, where it widens ir Asia, it partakes of the character of the climate of that c The continent of Asia extends, from east to west, beyond the of 70 degrees, over a space thirteen times as long as Euro northern coasts, throughout, touch not only the winter bou

the polar ices, but, except in a few points, and during a very short period of the year, their summer limits also. The north winds, the force of which in the open plains is not moderated by any chain of mountains to the west of the meridian of the lake Baikal as far as the 52nd degree of latitude, and to the west of the meridian of Bolor, as far as the 40th degree, pass over a field of ice covered with snow, which prolongs, as it were, the continent even to the pole; on the other side, Asia offers to the influence of the solar irradiation but a very small portion of country situated under the torrid zone between the meridians which bound its eastern and western extremities. The equator passes only through a few islands, Sumatra, Borneo, Celebes, and Gilololo; during the whole remainder of its vast extent, the equinoctial line cuts only the ocean: whence it results that the continental part of Asia under the temperate zone does not enjoy the effect of ascending currents similar to those which the position of Africa renders so advantageous to Europe. There are also other causes which tend to increase the frigidity of Asia; these are—1st, Its position with respect to Europe, which gives the latter all the western coasts, always under the temperate zones, much warmer than the eastern ones; 2nd, The form of its outlines, which, to the north of the parallel of 35°, present neither gulfs nor peninsular prolongations of any consequence; 3d, The form of its surface, which has, in one part, chains of mountains intercepting the approach of the south winds over a great extent of country, and in another, a series of high platforms lying in a direction from south-west to north-east, which, accumulating and preserving snow even in the midst of summer, act, by means of descending currents, on the countries which they bound or traverse, and thus lower their temperature. These contrasts between Europe and Asia present a summary of the causes which act simultaneously on the inflexions of the isothermal lines between the different seasons, and which are particularly perceptible to the east of the meridian of Petersburgh, where the continent of Europe joins Northern Asia in a width of 20 degrees of latitude. The east of Europe and the whole of Asia, to the north of the parallel of 35 degrees, have a climate eminently *continental*, as distinguished from the climate of the isles and the western coasts; they have, both from their form and their position with respect to the west and south-west winds, a *climate of excess* analogous to that of the United States of America, that is to say, very hot summers succeeding very severe winters. At Astracan M. de Humboldt has seen grapes as fine and as ripe as in Italy or the Canaries; although in the same spot, and even much more to the south, at Kislar, which is in the same latitude as Avignon, the thermometer (*Centigrade*) often descends in winter 28 and 30 degrees below zero. A more profound knowledge of the laws regulating the temperature of the earth in Asia, may produce a modification of the ideas entertained respecting the circumstances which have attended the last terrestrial revolutions. Thus, when it was known that the bones of

animals, the analogous species of which now exist only ii cal regions, are found still covered with the flesh in the d the plains in the north of Siberia, at the mouth of the Lena, banks of the Velhoui, between 72 and 64 degrees of nor it was immediately supposed that a sudden refrigeration o perature had, at some period, been operated in those cour this phenomenon appears now susceptible of being more plained by the cold which, as M. de Humboldt has a recently on the spot, exists in the earth, even in the midst o at a depth of five or six feet. When at noon, in the July and August, the air had a temperature of from 25 to 3 M. de Humboldt found, between 54 and 58 degrees of lat wells of small depth, which had not the slightest remains their borders, but the temperature of which varied from 2° above zero. M. Erman found, on the road from Tobol koutsk, in the latitude of 56°, springs at a temperature of 3° 8′ above zero, when the atmosphere was at 24°; but b parallel of 62° in the *steppes*, and even in the parallel of 60° not very elevated, the soil remains frozen at a depth of fr to fifteen feet. At Bogoslowsk, in the middle of sumn Humboldt found, at a depth of six feet, in a turfy soil, b shaded by trees, a bed of congealed earth 9½ feet thick, tra small fillets of ice, and containing groups of crystal of sc like a porphyritic rock. At Jakoutsk (latitude 62°) tl ranean ice is a general and perpetual phenomenon, notwit the high temperature of the atmosphere in July and Augu may easily be conceived, that from this parallel to that of t of the Lena, 72° N. latitude, the thickness of this bed of earth must rapidly augment.

These facts being established, it may also be remarked pical animals, tigers precisely similar to those of India, are in Siberia. Several tigers, of an enormous size, have be near the celebrated silver mine of Schlangenberg. Other which we now consider as peculiar to the torrid zone, have, as well as the bamboos, the ferns, the palm trees, and lithophyton, existed in the north of the ancient continent. probably, under the influence of the internal heat of the ear in the most northern regions communicated with the atmos through the crevices of the oxydized crust. As the atmos came chilled by the interruption of this communication, crevices were successively obstructed by interposed rocks, solid matters, the distribution of climate gradually becam entirely dependent on the solar irradiation, and the animal a table tribes, whose organization required an equal tempera more elevated degree, became gradually extinct. Some of hardy among the animals doubtless retired towards the so lived some time longer in regions nearer to the tropics; others the lions of ancient Greece, the royal tiger of Dzoungaria, the

irbis of Siberia, were enabled, by their organization and the effects of habit, to naturalize themselves in the climate of the centre of the temperate zone; some species even were enabled to inhabit the regions still more to the north, as M. Cuvier supposes was the case with the thick-haired *pachydermis*. Now if, during a Siberian summer, one of the last revolutions of the globe destroyed those elephants and rhinoceroses whose species is now lost, and which may be supposed to have been wandering at that season of the year towards the banks of the Velhoui and the mouth of the Lena, their bodies would find there, at the depth of a few feet, thick beds of congealed earth capable of preserving them from putrefaction. Slight convulsions, crevices of the soil, much less than those which we have seen in our days on the plain of Quito and the Indian Archipelago, would be sufficient to effect this embedding and preservation of the soft parts of those animals. The supposition of a sudden refrigeration appears, therefore, wholly unnecessary. It must not be forgotten, that the tiger, which we are in the habit of calling an animal of the torrid zone, now exists in Asia, from the extremity of Hindostan to Tarbagataï, the upper Irtychi, and the *steppes* of the Kirghises—an extent of forty degrees of latitude; and even sometimes in summer makes excursions one hundred leagues further to the north. Individuals of this species arriving in the north-east of Siberia, as far as the parallels of from 62° to 65°, might, by the effect of convulsion or crumbling of the earth, or other circumstances by no means very extraordinary, offer, in the present state of the Asiatic climates, phenomena of preservation very similar to those of the mammoth of Mr. Adams, and the rhinoceros of the Velhoui.

Preservation from Shipwreck.—On the 22d of August, M. Monnin presented a memoir on this subject. He proposes to fix round the vessels in stormy weather, large bladders made of the hides of oxen or horses, and filled with air, which would sustain the vessel and prevent its sinking, even when filled with water. He also proposes to diminish the dangers arising from vessels striking against rocks, by placing impermeable mattresses of hair or old linen between the coppering and the wood of the vessel.

Atmospheric Phenomena.—At the same meeting a letter was read from M. Jean Dufour, communicating a phenomenon observed at St. Serir (Candu). On the 20th instant, about five o'clock in the afternoon, the sun appeared round and white like a moon; that is to say, it emitted no apparent rays, and could be steadfastly regarded without dazzling or in any manner affecting the eyes. An hour afterwards it appeared of a pale blue colour, but still destitute of rays; and the horizon, at its setting, was of a deep red, such as is frequently observed after a very hot day. A kind of mist, at a considerable distance from the earth, and of trifling density, was uniformly spread in the upper regions of the atmosphere, and veiled the

sun. The thermometer marked 25 (Reaumur), and the
27 p. 4 l. The wind was easterly and the weather calm;
had not the stifling character peculiar to stormy weather
there any sign of thunder. During the day the objects e
the direct rays of the sun had been observed to assume a bl
M. Arago remarked, that from letters which he had recei
Perpignan and Bordeaux, it appears that the same atmo
phenomena were observed throughout the south of France.
sequent meetings various letters were received, which pro
the same phenomenon was visible in Italy, and several other
Europe. A similar phenomenon was mentioned by M. R
having occurred a few years since in South America.
conversation on this subject had terminated, M. Géoffroy St
read a letter which he had received from M. Lambert, of
miers, stating that at five o'clock in the morning of the 16th
the servant of M. Fimmerman, of the Château de Moral, sa
siderable volume of flame, unaccompanied by smoke, issue fi
ground at the foot of an old and large pear-tree. The sa
nomenon was witnessed on the same spot a few days afterw
another servant, and by M. Fimmerman himself. The sp
which the flame issued, presented the appearance of a hole
to that occasioned by the passage of gas which has forced i
after fire has exhausted a portion of inflammable matter. T
teau is situated at the base of a long chain of mountains, the
of which is about one hundred and fifty toises.

New Work by M. de Humboldt.—On the 12th of Sept
M. de Humboldt presented a new work to the Academy, e
'Fragmens Asiatiques.' The following is an analysis of its
tents:—General sketch of his voyage in Central Asia. Notice
discovery of diamonds on the western declivity of the Oural. Ac
of the quantity of gold and platina obtained from the Oural, be
1814 and 1830, presenting a total of 24,000 kilogrammes; a
average, during the later years, of 15,800 kilogrammes of gold
1700 of platina, mixed with osmium and iridium, per annum. Se
routes in Central Asia (from the southern frontier of Siberi
Kachkar, Yorkend, Ak-Sou, and Eachmis), collected at Semi
tensk, on the borders of Chinese Dyongaire, from a comparis
the accounts of various native Asiatic travellers. A notice o
astronomical position of several places in the south of Siberia,
of the position of the Chinese post of Rhonimailokhou, to the n
of Lake Dyayzan. A series of observations of magnetic inc
tions (the mean inclination of two needles) made during the jour
Considerations on the mountainous systems of Central Asia;
great depression of the soil around the Caspian Sea and the L
Aral, determined by barometrical observations; situation of volcan
which have emitted streams of lava at a distance of between th
hundred and four hundred leagues from the sea. Notice of the

and saltworks of Bakau, recently visited by M. de Lenz. Geographical additions by M. Klapproth, on the limits of the Atlas, after the Chinese authors, and of the volcanic phenomena of Central Asia. Arteaian and fire wells of the Chinese, at a depth of one thousand eight hundred feet, (perforated in a manner not yet used in Europe, by raising a beam or rammer with a cord, used by the Chinese from the most remote periods,) of hydrogen gas, both portable and brought by pipes from great distances for lighting, and for the evaporation of salt-waters. Ancient use in China of combustible bricks made of pounded pit-coal. Summary of volcanic phenomena considered in the most general point of view as the effect of the action of the interior fluid of a planet on its solid and oxydized exterior crust. Remarks on the progress of radiation at the surface, and on the interruption of the communications with the interior, which advance a state in which the relation of position with a central body (the sun) alone determines the difference in climate. Considerations on the temperature and the hygrometrical state of the atmosphere of the north of Asia. Effect of the subterranean ices on the preservation of the soft parts of animals, and remarks on the inutility of the geological hypothesis of an instantaneous refrigeration. General reflections on the causes of the inflections of the isothermic lines upon the numerical data of the distribution of heat on the surface of the soil, in the sea and in the air; upon a mode of arranging the mean results and examining the disturbing causes which are at first insulated, but afterwards accumulated on each other in such a manner as to disclose empyrical laws.

Statistics of Human Generation.—On the 29th of September, M. Mathieu read a report on a memoir by M. Giroux de Busaringues, containing a statistical account of the marriages, and the births of infants of both sexes, in France, classed in months. M. Giroux imagines the reproduction of man to be subjected to the same laws as that of domestic animals, and that whatever tends to increase the motive power of the man, or to diminish that of the woman, promotes the procreation of male children, and *vice versâ;* so that a man may render himself more or less apt to procreate boys or girls, according as he addicts himself to exercises productive of muscular force, or to slothfulness, to sobriety, or to intemperance. M. Giroux's facts are drawn from the official returns of every part of France, for ten years, commencing from 1817. He finds that, with respect to the number of marriages, the months are thus to be classed,—February, January, November, June, May, July, October, April, September, December, August, and March. This depends on the periods of religious festivals, and on those of rural labours, marriages being rare during those periods and most numerous in the months immediately preceding them. Thus March, the month in which Lent falls, is last on the list; while the preceding month, February, is first. The births of legitimate children are thus distributed,—February,

March, January, April, November, September, Decembe
May, August, June, July: counting backwards for nine r
have the following order as that of the conceptions,—I
April, July, February, December, March, January, August,
September, October. It will be observed, that the month
the greatest number of conceptions take place are not thos
there are the greatest number of marriages, which proves
ception rarely takes place in the first month of marria
average it attains its maximum about two or three mon
wards. The greatest number of conceptions are in spring
mer, and the smallest number in autumn and winter; but I
imagines that this is not so much owing to the direct in
the seasons, as to the fact of each season bringing period
recurrence of labours and habits more or less favourab
procreative power. Thus it appears that the greatest n
conceptions are in those months in which the strength o
developed by exercise and moderate labour; and the small
ber at the period of the dispersion and emigration of
population, and of the recurrence of more fatiguing labo
greatest number of boys are born in January and June, the
number of girls in December and July; whence the max
the conception of the former is in April and September, an
latter in March and October. M. Giroux is of opinion
female sex predominates in first conceptions. The greatest
of natural children are born in the months of February,
January, and April, and are, consequently, conceived in the
of May, June, April, and July; the same as has been obs
legitimate children. But the smallest number of births of
children is in the months of July, August, September, and O
and, consequently, the smallest number of conceptions is
months of October, November, December, and January, be
months further advanced in the cold season than the mini
conception of legitimate children. The greatest number
among natural children are born in January and July, and
in November and December; consequently, the most numero
ceptions of the former are in April and October, and of the l
February and March. M. Giroux has confirmed these genera
lations, by applying them to each department separately, an
the results nearly similar. The reporter concluded by recomm
M. Giroux to persevere in his researches after new facts to
and confirm the results at which he has arrived.

Wooden Houses.—On the 19th September, M. Navier m
report on a memoir by M. Blom, a colonel of engineers in S
relative to moveable wooden houses constructed by him. A
on a former memoir of M. Blom had been made last year by
Navier, de Prony, and Girard, in which it was suggested th
invention, although well adapted for cold countries, would be

open to objection in warmer climates. The present memoir of M. Blom has for its object the removal of this difficulty. He remarks that, with respect to the variation of temperature and the hygrometric state of the atmospheric air, it is hardly probable that the houses would ever be exposed to greater variations than in Sweden, where the thermometer of Reaumur in July generally marks 20°, and sometimes 24°, above zero, and falls in winter to the same number of degrees below zero. In the latter case, the temperature of the interior of the houses is preserved, by stoves, at from 12° to 19° above zero. M. Blom points out particularly the advantage derived from the construction of the wooden walls, both on account of their small thickness and their small conducting power: thus, in thaws, every part of the edifice promptly assumes the highest temperature of the atmosphere, and there is not that precipitation of water which is always observed on the surface of stone walls, and which produces a very injurious humidity. It is well known that the Swedish ships remain a long time in hot countries without being at all injured by the heat of the sun, although the surfaces of the decks are not covered with coating; and it appears that the moveable houses of M. Blom have been used for four years in the Swedish colony of St. Bartholomew, and no complaints have been made of their having sustained injury either from the heat or the hurricanes which prevail there. These houses are warmed by portable stoves, also invented by M. Blom, and they are insured against fire at the ordinary premium, the offices having been satisfied that, while they are not more liable to conflagration than ordinary houses built of the same kind of wood, they have the advantage of being easily removed out of the reach of danger. Every part of these houses is moveable, so that, with very little expense, the form and position of the rooms may be changed at pleasure. In countries subject to earthquakes, these edifices are particularly desirable, as, independently of the facility of removal, they are much less likely to be destroyed or overthrown than constructions in stone. But it is in new colonies that their advantage would be most sensibly felt. Wherever it may appear to the settler desirable to fix his residence, his habitation is ready, and may be removed elsewhere when circumstances may render a change desirable. Large public edifices, such as hospitals, barracks, prisons, &c., may in like manner be easily transported wherever a change or increase in the territory may render it desirable to remove the seat of government. As far as the mode of construction can be understood from the drawings appended to M. Blom's memoir, it appears that the walls are formed of thick planks, two together, and united at their joints by keys. The planks in the outer row, which are the thickest, are placed upright or vertically; those in the inner row are placed horizontally. Between the two rows is interposed a species of pasteboard, impregnated with bituminous substances. The joints of the principal pieces, which form the angles of the roofs and floors, are secured by buttons; the angles of the

panels which form the walls and partitions are united and in the same manner. The author states that he intends to house to France; and it is very desirable that he should do is impossible to form an accurate idea of the details of the co tion from drawings. It appears that success in constructin houses depends in a great measure on the excellence of the wood, as well as on the care taken in selecting and prepar wood proper for the different parts of the edifice, and the nic precision of the work; other architects may, therefore, at fi considerable difficulty in constructing them. It is unquest that the mode of building employed by M. Blom is immea superior to that hitherto in use in Sweden and Russia. In sion, the reporter strongly recommended M. Blom to the a tion of the Academy, as having invented and brought to pe a new and useful branch of industry, which it would be desi have known in France.

FOREIGN AND MISCELLANEOUS INTELLIGE

§ I.—*MECHANICAL SCIENCE.*

1. On the Flexure and Force of certain Wood

Experiments have been made by Mr. Brown, at Newport Island), U.S., on the resistance to flexure belonging to tl woods,—*pin du lord* (*Pinus strobus*), spruce fir (*Abies nigr* southern fir (*Pinus australis*, *Pinus longifolia*): these were numbers 120, 132, 193. The third kind, therefore, deserves t ference and choice of those planters who, especially in large tions, prefer real utility to beauty. The *pin du lord* is con worthy of its place in parks; and, for the improvement of the southern fir may be mingled with the other fir-trees; but it will be more valuable than the Scotch fir (*Pinus sylvestris*) Corsican fir, for size, straightness, and strength, can only be tained by similar experiments to those above, and observation its growth. It may probably be a very valuable addition from rica to the firs already known in Europe*.

2. Double Images of Objects seen through the Ai

M. Rozet has frequently observed double images of objects seen tl the air; he compares them to the images formed by doubly refi spar, and goes so far as to say that the atmosphere has the prop

* Revue Ency., i. p. 173.

occasionally giving two images, nearly as Iceland spar. During his residence in Africa, the same phenomenon was presented in a very remarkable manner at various times, particularly at the camp of Staonelli, on the 27th June, 1830. At 10 o'clock, A.M., the sky was very clear, and the thermometer at 21° R. (80° F.) On looking at the line of battle, formed before the camp, there were distinctly two images. The extraordinary image was not so strong as the other, but yet perfectly distinct from it; it was raised about a fourth of the height of the objects (query, *men?*) and deviated very slightly in a lateral direction. The same effect occurred with isolated men. Many Algerine tents in the hands of the French had on their summits spheres of tinned iron, surmounted by a crescent. On all these spheres was seen a second, tangential to the principal one; so that it seemed at first as if there were two.

Whether the two images were repetitions of each other in the same direction, as is the case with Iceland spar; or whether there was an inversion of any part of the images, as happens in all ordinary atmospheric refractions, M. Rozet has not mentioned, although the distinction is a very important one to the analogy referred to between the action of the air and doubly refracting bodies. The observations were read to the Royal Academy of Sciences at Paris, on the 20th June*.

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§ II.—*CHEMICAL SCIENCE.*

1. On the Rapid Production of Steam by heated Metals.

Some highly interesting and practical experiments have been made by Professor Johnson, of Philadelphia, on the quantity of steam evolved, and time required by heated metals. There is every reason to believe that explosions have often happened, especially on board vessels, by the water being either splashed or returning over parts of the boiler which have been highly heated; and in the arrangement of the boilers in the American steam-boats this is especially likely to be the case. Hence it becomes important to know what power of suddenly raising steam from boiling water such heated iron would possess; and this was done, in the experiments, by plunging the metal into a certain portion of weighed water at 212°, contained in a vessel, itself guarded by a coat of green-baize, cotton, &c., so as to prevent loss of heat, and attached to a scale-beam. The vessel could hold 28½ lbs. of water at 60°; when 14 lbs. of boiling water were put in, it required 14 hours for the temperature to sink to 115°, the temperature of the place being 80°.

When used, 15 lbs. of water were put in the vessel suspended to

* Revue Ency., i. p. 618.

the scale-beam, and the water and vessel raised to 212° by
On making the experiments, the hot metal was introduced
once or more gradually, covered over with a perforated co
the metal always withdrawn upon the cessation of ebulliti
loss of weight was then ascertained. As it was difficult to a
temperatures above the boiling point of mercury, a *barely*
in daylight was chosen as a standard of comparison betw
different metals and different masses of the same metal; an
probable that the heated parts of boilers are seldom raised
dull red heat, it was thought that, for practical as well as th
purposes, that point would be most interesting and importan
experiments to determine the period of *greatest activity* sh
just below the point of visible redness in daylight, the greate
tity of steam is generated in a given number of instants;
least, is the case when the experiment is performed under
atmospheric pressure, and this point, therefore, has been ter
comparable temperature.

The following is a table of experiments with rolled iron
plate, 25½ inches long by 7½ broad and $\frac{3}{16}$ths thick, exposi
square inches of surface, and rolled into an open coil. Th
was at 212°, the barometer at 29.9 inches: the room was fr
to 85°.

Weight of Metal in oz. avoird.	Time in Seconds.	Oz. avoird. of Steam.	Steam frm. each oz. of Metal.	Oz. Metal for one oz. of Steam.	Observed Heat in Daylight.
144	40	10.75	.0746	13.395	Black heat.
144.25	90	16	.1169	9.016	*Comparable*, or dull red in da
144.25	90	16	.1109	9.016	Ditto.
144.125	90	16	.1110	9.008	Ditto.
144.125	90	16	.1110	9.008	Ditto.
144	70	16.5	.1145	8.727	Rather hotter; plunged soon
144	150	19.75	.1371	7.291	Clear red; immersed by deg
144.25	120	20	.1386	7.2125	Bright red.
144	90	21	.1458	6.857	Brighter red.
144	90	22.5	.1562	6.400	Very bright; metal yielding

The coincidence of the second, third, fourth, and fifth expe
is remarkable, and proves that, at the temperature of comp
9 lbs. of wrought-iron will generate 1 lb. of steam under atmo
pressure. In after experiments it was found that, but for the
taken to avoid waste and error, this effect might have been pr
in 25 or 30 seconds instead of the times noted.

A second series of experiments were then made with wr
iron cylinders 6 inches long, 1.7 in diameter, having a surf
38 square inches, including the hook; the water being, as
at 212°.

Weight of Metal in oz. avoird.	Time in Seconds.	Oz. avoird. of Steam.	Steam frm. each oz. of Metal.	Oz. Metal for one oz. of Steam.	Observed Heat in Daylight.
62.5	42	4	.0640	15.625	Black; iron immersed at once.
62.5	45	4	.0640	15.625	Ditto.
62.5	45	5.25	.0840	11.904	Ditto.
62.5	48	5.5	.0880	11.363	Ditto.
63	120	7	.1111	9	Dull red; *comparable;* immersed by degrees.
63	120	7	.1111	9	Ditto.
63	120	7	.1111	9	Ditto.
63	120	7	.1111	9	Ditto.
63	80	7.25	.1150	8.689	Ditto; immersed quickly.
62.5	90	7.75	.1240	8.064	Fair red; ditto.
63	150	8	.1270	7.875	Ditto; immersed by degrees.
63	150	8	.1270	7.875	Ditto; ditto.
62.25	100	9.5	.1365	6.552	Full red; immersed at once.
62.25	120	10.5	.1686	5.928	Bright red; ditto.

Experiments five, six, seven, eight, and nine correspond closely with two, three, four, and five of the last table, and show that, at the comparative temperature, iron in this form, as well as in plate, produces 1 lb. of steam for 9 lbs. of metal.

By a third series of experiments, with cast-iron, it was found that, at the comparable temperature, it could generate more steam than wrought-iron,—8½ lbs. nearly being sufficient to produce 1 lb. of steam. This effect is, perhaps, attributable to a difference in the specific heat of the two substances, which may well exist, considering the difference of their composition*.

2. Discharge of Lightning over a large Surface.

The following interesting electrical account is given by Mr. Bryant, clerk in the State Prison at Charles Town, Massachusetts: 'Yesterday,' July 30, 1829, 'we had a severe shock of lightning at the prison. It rained in torrents, and a dense mass of highly-charged clouds spent their embosomed electricity on and about us. I was looking out of my office window to discover the direction in which the clouds were moving, when a flash, accompanied by a rustling noise like that of small shot thrown upon stiff paper, and a feeling as if all the energy of my muscles was at once withdrawn, and an almost insuperable inclination to fall back on the floor, convinced me that I had been struck with lightning. But I only tottered back a few steps, and recovered myself immediately. On leaving my office to inquire what mischief had been done, I learned of the officers that almost all of them, as well as many of the convicts, had been affected like myself. My office is in the brick building directly south, and in front of the prison, about three hundred and sixty yards from the north wall of the prison yard. Between the office and the prison building is a large yard, perhaps one hun-

* Silliman's Journal, xix. 292.

dred and fifty feet wide, the length enclosed by the wall hundred and eighty feet, the width three hundred and sixt The prison has three conductors on it, about equidistant fro other, say eighteen feet. The lightning passed down each o without any injury to the building, except starting a few slat the ridge post. Now what appears singular in this case is, person out of nearly three hundred officers and convicts was in t injured, although almost every one was more or less affected Nearly all of these persons had either a steeled hammer, a with bayonet fixed, or some metallic utensil in their hands. a yard of my situation is an armory with thirty guns and as steel-pointed pikes, the points and bayonets pointing up. account for our escape only by supposing that the fluid was at by so many different objects on all parts of the building, over the yard, that it divided itself just before it reached passed off in such small quantities as almost to lose its effec is singular that men standing five hundred feet distant fr should be affected in the same degree. I suppose that one h tons of iron are exposed on the different buildings in grates, pillars, &c. &c. One of the officers had a saw in his hand, he says, seemed to be "light red fire." Another was stoopi picking up nails from the floor, and the instant after the flash himself standing bolt upright, with his hands tightly clench gether. The effects of this shock were felt over a surface hundred and seventy-two thousand five hundred feet in nea same degree, without any permanent injury being sustained*.'

3. Becquerel on the Electric Effects produced by] and Pressure.

M. Becquerel has lately examined the changes which take pl the electric state of bodies by the action of heat, contact, pressu of which the following is an abstract:—It has been long know if a body, which receives positive electricity by friction with a be raised to a high temperature, it gradually loses this power; a certain temperature receives negative electricity.

Iceland spar, which is positive when rubbed with any subs becomes negative when its temperature is sufficiently raised. author expected to find the same property in different meta garded as electro-positive and electro-negative. He expecte caloric, by separating the atoms of metals, would produce s effects with cleavage in crystallized bodies. When the plates of or sulphate of lime, are suddenly separated by cleavage, the thus separated are found to be in opposite electric states. I parts thus separated be again pressed together and suddenly rated, they are again found in the same electric states, an difference in those states is more marked as the temperatu

* Silliman's Journal, xvii. 193.

raised. By means of a torsion-balance, and a press for compressing bodies, the author has arrived at some curious results. He found that the excess of electricity acquired by each body was proportional to the degree of pressure to which it was submitted. If, for example, a thin plate of mica and a thin disc of cork be pressed together with a certain force, and then separated, and the electric tension ascertained, and if the experiment be repeated with a double pressure, the electric tension will also be double. The author found, by submitting the discs, first to a great pressure, and without separating them, to a smaller, that the effects of the great pressure remained for some time; and when the discs were separated, they were found to possess a higher electric tension than that which belonged to the least pressure. These remarks only apply to bad conductors of electricity.

The author has demonstrated that heat has no effect on free electricity, but has a considerable effect on the neutral fluid naturally belonging to metallic bodies. If one end of a metallic rod is heated, that extremity manifests positive electricity, whilst the other end exhibits the negative state. The author compares the series of decompositions and recompositions of the two elements of the neutral fluid along the metallic rod, to the transmission of heat from one molecule to another by conduction. He then shows how the electricity thus developed by heat may be removed, and the opposite state rendered evident. Having rolled the end of a platina wire in a spiral form, and placed it on the cap of a gold cup electrometer, (metallic contact being prevented,) he applied the flame of a spirit lamp to the spiral which projected over the cap of the instrument, till it was raised to a red heat. The lamp being removed, and the spiral touched with a piece of moist paper on a heated rod of glass, the positive electricity was removed, and the gold leaves diverged with negative electricity. M. Dessaignes discovered long ago, that if the end of a plate of silver be heated, and the two ends brought in contact with the nerves and muscles of a frog, that contractions were produced. Mr. Ritchie has shown, in the Philosophical Transactions, that a powerful electro-magnetic effect could be produced by two pieces of the same metal, having one of the ends raised to a high temperature; but this experiment of the author is the only one in which electric tension, capable of causing divergence in conductors by heat alone, has been observed. The author tried experiments with the more oxidable metals, and found them to possess the same property, though in a less degree, and that in bismuth, tin, and antimony, the effects were scarcely sensible. The second part of this memoir contains theoretical views of the electric and chemcial theory, which the curious in such matters can examine in the original.

4. Crystallization of Perchloric Acid. (*Serullas.*)

Solution of perchloric acid was concentrated by evaporation until

it evolved white vapours in some abundance; it was then : four or five times its volume of concentrated sulphuric aci tilled in a small retort, to which a receiver had been adaj mixture became coloured, and when boiling, evolved ch oxygen; but a small quantity of the perchloric acid escaj position, rose in vapours, and condensed in the receiver, purposely cooled. Thus distilled, this acid is solid, it c sulphuric acid; exposed to air, it rapidly attracts water, same time evolves dense white fumes. When melted and p water, each drop hisses like hot iron. It melts at 45° C. It appears in two forms, either massive, or in long pris: rently quadrangular, and terminated by dihedral summits : form is, without doubt, that which contains the *minimun* and is consequently the most volatile.

Several precautions are required in procuring with cer *crystallized* acid. The sulphuric and perchloric acid are troduced successively by a long tube into a small retort lated, the neck of which is to be introduced (without a co: bent tube contracted at the extremity. On applying heat, boils, and is by little fire preserved in that condition; a porti acid soon flows slowly over and solidifies in the tube, w sufficient to cool with water. The process must be stopp the mixture is discoloured, and as soon as a drop of liqui over and retains its fluid state, otherwise water will dist: dissolve the crystals forming the liquid and non-fuming ac the same reason, only small quantities should be operat once, not surpassing eight or ten grammes (140 gr.) of p acid.

Perchloric acid may be concentrated in a retort, by car like sulphuric acid: the first portions are merely water. It been carried to a s. g. of 1.65, and may perhaps be urged In this state it evolves some vapour in the air: it boils at (392° F.) If, when boiling, dry paper be held in its vapc to the aperture of the vessel, it inflames vividly. Of this ten parts exposed to air attracted 1·8 parts of water in twe hours, and in ten days had attracted 8 parts*.

5. Strength Test for Bleaching Powder.

The necessity of having a means of ascertaining the strength of bleaching powder has been felt so strongly, th persons have turned their attention to the discovery of an u: tionable process for the purpose; and the use of sulphate of of salts of manganese, and of the chlorometer apparatus Lussac, is consequently well known to all who are concerne use of that chemical production. M. Marozeau, amongst has sought to obviate the objections belonging to all the pr

* Ann. de Chim., xlvi. 294.

known, and has described, as the result of his exertions, a new process founded on the use of mercurial salts. Let muriatic acid be added to a solution of protonitrate of mercury, in quantity more than sufficient to precipitate all the mercury as calomel; then let a solution of chloride of lime be added: the chlorine set at liberty by the excess of acid will react on the calomel, will convert it into corrosive sublimate, which, dissolving, the solution will become perfectly clear and transparent again, if enough chloride of lime has been added.

This effect, when produced by known solutions of mercury and bleaching powder, and with the attention required to obtain a complete chemical action, is said by M. Marozeau to furnish a very excellent method of ascertaining the strength of bleaching powder: for by agitation of the liquids, all the calomel at first formed may be converted into corrosive sublimate, and dissolved before the slightest odour of chlorine is sensible in the residual liquor. He uses the chlorometer of M. Gay Lussac, but inverts the office of the pipette or fixed measure of bulk: instead of using it to measure out the bulk of solution of chlorine to be tried, it is employed to measure out a fixed quantity of the test solution of nitrate of mercury, and the graduated jar is used to ascertain the quantity of solution of chloride required to convert the calomel when formed into corrosive sublimate.

The strengths of the solutions of nitrate of mercury and bleaching powder to be tried, are made to conform to the dimensions of the instruments constituting Gay Lussac's chlorometer. The proof-liquor is procured by boiling mercury in excess in dilute nitric acid, continuing the ebullition until no deutonitrate remains in solution. The strength is adjusted in two ways, either by preparing a solution of chloride of lime with a known quantity of chlorine, and then trying it against the test solution as yet unadjusted, and diluting the latter until it agrees with this known solution,—or by ascertaining how much of the test liquor is required to precipitate the whole of the chlorine in a known solution of common salt. For, as the quantity of chlorine in common salt required to convert the mercury in a given quantity of test solution into calomel, is exactly equal to that required afterwards from chloride of lime to convert the calomel so formed into corrosive sublimate, it is easy to make a known solution of salt, and to dilute the test liquor, until a given quantity of it will exactly precipitate a measure of that saline solution; and such test liquor will, by the process recommended, show what quantity of the solution of bleaching powder contains the same proportion of chlorine as the standard solution of salt thus referred to.

M. Marozeau then gives minute instructions for the use of this process, intended for those who, not possessing much chemical knowledge, still have to apply the instrument; and he states that, having used it very constantly, it has afforded him highly satisfactory results *.

* Ann. de Chim., xlvi. 400.

6. Preparation of Iodic Acid.

The following is a method of preparing this acid, d recommended by A. Connell, A.M.:—The vessel em a rather large and tall flask, into which fifty grains of an ounce of fuming nitric acid were put; the acid was ma and as soon as any iodine sublimed and condensed on t the vessel, it was washed back again into the liquid by After the process had been continued some time, a preci white crystalline grains was observed to take place, and tion of boiling and washing back the sublimed iodine was until the free iodine had, to a great extent, disappea whole was then decanted into a shallow basin, and evap dryness.

Any free iodine which had remained was soon dissipat heat. The residue of the evaporation consisted of whitish grains, which were iodic acid, retaining a little nitric a which they appeared to be freed by one or two solutions and re-evaporations, when they lost most of their crystallin ance, and became a whitish deliquescent mass, occasional light purplish tint, from a tendency to decomposition by th evaporation. Where no particular precautions were taken t loss in the state of vapour, and where the process was not c until the entire disappearance of iodine, the quantity of tained approached that of the iodine employed; a larger p of iodine might probably be used with the same quantity of

7. Method of Marking Linen.

The necessity of marking the linen of hospitals, &c., in a and durable manner, so as to resist the action of alkalies, sc is so important as to have induced M. Henry to examine the in use, and endeavour to replace them by a better. The and muriate of manganese, the sulphate and acetate of iron of silver, acetate of alumine and iron, and acetate of lead, mi gum or indigo, or ink, have been used for the purpose; but al require previous or subsequent operations of some nicety, as sion in carbonated alkalies or hydro-sulphurets, or else such of care as to be inexpedient in the hands of the women or to whom the duty generally devolves.

The following is the process which M. Henry ultimately mends as the very best. Take one part, by weight, of iron and three parts of vinegar, or acetic acid of s. g. 1056. M filings with half the vinegar, and agitate it continually. thickens, add the rest of the vinegar, and also one part of Then apply heat to assist the action, and when all the iron solved, add three parts of sulphate of iron, and one part of arabic, previously dissolved in four parts of water. These are

* Jameson's Jour., 1831, p. 72.

mixed well at a gentle heat, and will yield twelve parts of the preparation.

When to be used, the linen is to be spread on a table, and the preparation applied by means of a hair brush, and stencil plates of copper*.

8. New Applications of Artificial Ultramarine.

It is well known that a few years since M. Guimet discovered a process of manufacturing ultramarine from its proximate elements, and without the use of lapis lazuli. He has latterly described a great extension of this, his manufacture, in a letter to M. Gay Lussac. A paper manufacturer wished to apply this ultramarine in place of smalt to the coloration of his papers, and was, in consequence, supplied with a sufficient quantity to make a large experiment. The latter paper made had as good a tint as that coloured with smalt, and was more uniform, but it was found that, in producing this effect, the 1lb. of ultramarine, because of its extreme division and intense colour, was as effectual as 10lb. of the finest smalts. After this 200lb. of ultramarine were sold to the paper-makers of Lyons at the price of 20 francs per lb., and proved to be more economical than smalt. In consequence, M. Guimet has very much extended his manufactory, and is able to sell ultramarine for these uses at the price of 16 francs per lb.

The ultramarine for painters requires a particular purification, as well as careful selection from all that is manufactured. The price for the finest quality is 60 francs the lb. The second quality is 20 francs the lb.

Besides its use in paper-making, the manufacturers of calicoes, muslins, &c., &c., are beginning to use it, and M. Guimet expresses a hope, that shortly France will be entirely independent of other countries for the blues required for these uses *.

9. Reduction of Titanium.—(*Liebig.*)

Recently prepared chloride of titanium and ammonia (of Rose) is to be introduced into a glass tube, two or three feet long and half an inch in diameter, so as, without being pressed, to occupy the half of it. The tube is to be placed horizontally in a furnace, and attached to an apparatus, by means of which, ammoniacal gas, dried by passing over caustic potassa, may be supplied. The vacant parts of the tube are first to be heated whilst a little ammonia is passed in: gradually the part containing the salt is to be heated and the temperature raised until the tube softens. The chloride is entirely reduced, and the tube being opened, when cold the metal is taken out in the form of a dark blue powder, or in plates, having the lustre of copper. If exposed to the air before it is cold, it will inflame and burn into titanic acid.

* Jour. de Pharm., 1831, p. 388. † Ann. de Chimie, xlvi. 481.

When the sublimed chloride is used, the metal is c
beautiful crystalline groups.

The parts of the tube which are last heated are frequer
up with muriate of ammonia: it is useful, therefore, to
smaller tube, and clear away the muriate from time to
attaches to it.

Perhaps tungsten and molybdenum may be thus reduc
similar trials upon silica and alumina have failed *.

10. Preparation of Metallic Cromium.—(*Liei*

When ammoniacal gas is passed, in a similar manner to
over the chloride of chromium and ammonia, heated to r
glass tube, metallic chromium is obtained as a black]
metallic powder, assuming lustre when burnished, inflami
heat, and burning into a brown powder.

When ammoniacal gas is passed over the chloride of chr
combination occurs with the disengagement of light, th
filled with a purplish red flame, which continues until the
saturated. When the chloride of chromium is heated in ar
gas, the metal is also obtained, but is then of a brown
colour instead of black.

When in the preparation of chloride of chromium the ne
tion of muriate of chromium is evaporated, a green mass is
which evolves no water at temperatures even a little abov
But at temperatures of 400° or 500° F., it begins to s
losing water, becomes a brilliant spongy crystalline peach
mass, perfectly fixed in the fire. The conversion of a muri
chloride cannot be so convincingly shown on any other subs

When the chloride is heated in the air, a very beautiful
chrome, as to colour, is obtained, fit for porcelain works.

When chloride of chromium is heated in sulphuretted hyc
crystalline brilliant black sulphuret of chromium is procure

Metallic chromium, prepared as described, burns, if calcine
air, but does not become of a green colour: the oxide thus]
has not been examined, to ascertain whether it be the same
a different oxide from the green one †.

11. Analysis of some Mercurial Salts.

Mr. Phillips has recently examined and analysed some of t
of mercury, especially some sulphates and carbonates.
two parts of mercury and three of sulphuric acid are heate
short time, some protosulphate of mercury is formed, but c
tinuing the heat, almost the whole becomes bipersulphate.
being put into water, is decomposed, and the yellow precipita
merly called turpeth mineral, thrown down. When 200 p
the bipersulphate were put into water, 141.1 parts, and then

* Ann. de Chimie, xlvii. 108.

† Ibid., p. 110.

8.4 parts, more of the yellow precipitate were obtained. On analysing this precipitate, the mean of experiments gave sulphuric acid 12.6, and peroxide of mercury 87.5 per cent.; from which it would appear that the salt is a subpersulphate, constituted of—

3 atoms sulphuric acid	. . . .	120	or	12.2
4 ——— peroxide	. . . . .	864		87.8
		984		100.0

The acid and oxide remaining in the solution have been supposed to constitute a peculiar supersalt; but when 4 atoms of bipersulphate of mercury are acted on by water, 3 atoms of acid and 4 of oxide are precipitated, whilst 5 of acid remain in solution; this dissolving a part of the bipersulphate prevents decomposition of the whole, and the quantity remaining in solution depends, to a certain extent, upon the quantity of water used.

The carbonates of mercury were then examined. Carbonate of potassa, added to protonitrate of mercury, produces at first a yellow, but, when in excess, a black precipitate. The yellow precipitate dissolves in acid without effervescence, and was a subprotonitrate; the black precipitate, when dried by exposure to air, was only black oxide.

By adding carbonate of potassa to pernitrate of mercury, a precipitate with an ochre yellow colour was obtained, which being dried by exposure to air, and then dissolved in nitric acid, lost 4.4 per cent. of carbonic acid; the solution, decomposed by soda, gave 96.1 of peroxide of mercury. The salt is therefore a dipercarbonate, consisting of—

2 atoms peroxide	. . . . .	432	or	95.2
1 ——— carbonic acid	. . . .	22		4.8
		454		100.0 *

12. On the Preparation of the Iodides of Mercury.

These compounds may be prepared either by precipating proto or persalts of mercury by iodide of potassium, or by triturating iodine and mercury together. The former has many inconveniences in consequence of the effects produced by excess of either precipitate, or by changes effected on the iodide itself by different circumstances influential during its formation. M. Berthemot, therefore, very much prefers the latter, which, a little modified according to his suggestion, affords very excellent iodide of mercury.

The protiodide consists of single proportionals of the elements, consequently, according to Berzelius, of mercury 1265.822 and iodine 789.145, or per cent. of 61.6 and 38.4. These weights are therefore to be taken, put into a mortar with a flat bottom, and triturated together. The mixture soon takes a reddish colour, and upon adding a few drops of the very strongest alcohol, and continuing to

* Phil. Mag. N. S., x. 205.

triturate it, will become yellowish green, and all the mercury a iodine will have disappeared. The alcohol will be almost entire evaporated, and a very fine protiodide will have been produced. this operation there is formed at first deutiodide mixed with iodine a mercury; the alcohol, by dividing the deutiodide and dissolving t iodine, so as to form a very concentrated solution, brings the wh into mutual contact, and very promptly determines the combinati of the two bodies.

When 100 parts of the deutiodide, and 44.5 of metallic mercu are triturated with a little alcohol, protiodide is also readily formed

Deutiodide of mercury consists of 1 proportional of mercury t of iodine, or of mercury 1265.822, and iodine 1578.29, or per ce 44.51 and 55.49. These proportions being mixed are to be tri rated as before, but the alcohol must be added drop by drop, and t trituration continued a few moments between each addition. T addition of too much alcohol renders the action so strong, that mu heat is evolved, the mixture even fuses, and the iodine is vapouri and lost. This process succeeds very well, and gives a compoun known and definite composition, although the appearance is not beautiful, nor the colour so fine, as that prepared by precipitati This is due, without doubt, to the particular state of aggregat and arrangement which the particles acquire in precipitation.] when both are made to undergo sublimation, they become alike every respect *.

18. On Borate of Silver.—(*Rose.*)

When a concentrated solution of borax (either fused or crys lized) is mingled with a moderately strong solution of nitrat silver, a white precipitate of borate of silver falls. Whichever the solutions are mingled the same effect takes place. When w is gradually added to this precipitate, it dissolves entirely, like r of the precipitates produced by alkaline borates, for very few ap to be insoluble in water. Water produces no change in the co tution of this substance; light renders it violet or black. W washed as well as may be, it was analysed, and its constitu appeared to be—

Oxide of silver	76.9
Boracic acid	23.1
	100.0

Here the acid contains only thrice the oxygen of the oxide, b borax the acid contains six times the oxygen of the base. If, th fore, borax be a neutral salt, this argentiferous compound is a sul

When a saturated solution of borax is diluted thirty or forty ti so that the water present shall be enough to dissolve any bora silver formed, one of the solutions being in excess and the two b

* Journ. de Pharm. 1831, 456.

poured indifferently to each other, a precipitate is obtained quite different from the borate of silver. It is brown, insoluble in water, and when well washed proves to be oxide of silver; by heat it loses 9 per cent. of oxygen and water.

Thus, whilst a concentrated solution of borax produces a subsalt from a solution of silver, the effect of the boracic acid is entirely lost by mere dilution, and the solution acts, with respect to silver salts, as pure alkali. This effect of water cannot be compared to that upon bismuth salts or verdigris, for the subborate of silver is entirely soluble in water.

Solution of borate of potassa acts in the same manner. Borate of ammonia in strong solution gives, with nitrate of silver, a white soluble precipitate, and, when diluted, it produces no precipitation. Sulphate of silver acts in the same manner as the nitrate *.

14. Igniting Platina.

Dr. Hare says, 'I find that if asbestos or charcoal be soaked under an exhausted receiver in muriate of platina, then dried in an evaporating oven for twenty-four hours, and afterwards ignited, the property of ignition in the gaseous elements of water is acquired †.'

15. On organic Analysis, and on the Constituents of various organized Bodies.

MM. Henry and Plisson have been very earnestly employed for some time past in endeavouring to improve the means of analysing organic bodies, so as to obtain accurate estimates of the proportions of their elements, and for this purpose have exerted themselves to give the element under consideration, whether oxygen, hydrogen, carbon, or azote, a gaseous form, conceiving that the probable errors in the mensuration of gases are far smaller than those likely to occur in the estimation of weights. Their processes have been published in several memoirs in the Journal de Pharmacie, and elsewhere, and they have finally given an account of the results of many analyses obtained by their methods, which it is our intention to abstract or transcribe.

The substances were used in the purest possible state; for they were—i. Prepared with great care and crystallized many times. ii. They were calcined to ascertain the absence of inorganic matter. iii. They were carefully dried at 212° F. iv. The mixtures of the principles and oxide of copper were always preceded in the tube by an extensive portion of pure oxide of copper, heated powerfully, so as to decompose the carburetted hydrogen and oil formed upon the first action of heat on the destructible matter. v. The tube and compound were always placed in the most favourable circumstances for the avoidance of errors.

* Ann. de Chimie, xlvi. 319. † Silliman's Jour., xx. 160.

Carbon—Has always been estimated in the state of carbonic aci and when heat has been applied and the oxide of copper has dc its duty, pure oxygen has been passed through the tube to burn wl carbon might remain in contact with the reduced copper: this p caution appears very necessary; 100 of carbonic acid was conside as containing 27.6508 of carbon.

Hydrogen—Was occasionally obtained in the gaseous state, bringing the water produced in the analysis in contact with the al of antimony and potassa, first cold, and then heated; but as u various trials the process of estimating its quantity from the weigl water produced was found to be exact, it was generally adopted.

Azote.—This principle was obtained in the gaseous state by pas the nitrous oxides and acid over heated sulphuret of barium (sulpl of baryta heated with charcoal); the oxygen was taken away and nitrogen sent forward into the receiver; but that no hyponitrit baryta might be formed from nitrous acid, some pieces of met copper were interposed between the oxide of copper and the sulpl of barium: this would convert any nitrous acid into nitric oxide, prevent the source of error referred to.

The following are the proportions of the elements containe many substances, each being in a very pure and dry state.

		Carbon.	Hydrogen.	Oxygen.	Nitrogen
1	Mannite.....	0.38770	0.08487	0.52743	0
2	Cantharidine ·	0.68560	0.08432	0.13152	0.0985
3	Piperine	0.76100	0.10274	0.13626	0
4	Caryophylline	0.81920	0.12250	0.05730	0
5	Aurade......	0.83760	0.150892	0.011508	0
6	Amynine	0.8104000	0.1047368	0.0848632	0
7	Ceroxyline ..	0.83200	0.11050	0.05750	0
8	Arbre à brai. .	0.797280	0.106512	0.096208	0
9	Alouchi	0.8264000	0.1100624	0.0635376	0
10	Vol. Oil of Copalmi balsam	0.8925	0.1046	0.0029	0
11	Bitter almonds	0.744000	0.0683452	0.1179268	0.059
12	Black mustard	0.532800	0.111840	0.094416	0.149
13	Morphia	0.705200	0.079884	0.167056	0.047
14	Quinia	0.745520	0.084322	0.087212	0.082
15	Cinchonia....	0.788800	0.088760	0.028918	0.093
16	Strychnia	0.764000	0.078784	0.082190	0.075
17	Brucia	0.704800	0.078108	0.149154	0.067

Aurade (5) was discovered in the volatile oil of orange flar one of the authors. 6, 7, 8, and 9 are subresins discovered Bonastre, and described in the Journals some time since. I nastre supplied those which were analysed. There is reason from the oxygen in the volatile oil of bitter almonds, that had acted on it, although great care was taken to prevent the c

* Sulphur 0.111792.

With regard to the vegeto-alkalies 13, 14, 15, 16, and 17, every possible care was taken to obtain them in a state of purity, and especially free the one from another. The occurrence of several of them in conjunction rendered the latter precaution exceedingly necessary, and the authors suggest that probably the discordant results already published arise from some such mixture *.

16. Results obtained from the Seed of the Mango.

The mango tree (*Mangifera Indica*, L.) has been transported from the East Indies to St. Domingo, and the other neighbouring islands, where it is now exceedingly abundant. In consequence of which its products may now find useful applications; to forward which purpose M. Arequin has devoted his attention to the analysis of the seed. The fruit is a fine mass of pulp, very agreeable in the estimation of some, and the seed or grain lies in the middle, having the form of a kidney, and inclosed in a parchment-like integument.

The mango pulp contains much crystallizable sugar, and also citric acid and gum.

The mango-seed is remarkable for the large quantity of gallic acid present, and for the presence also of stearic acid, and for the useful state of its starch. When a seed is cut with a knife, it gives a deep blue colour to the latter; when touched with persulphate of iron, it acquires a fine blue colour, both effects due to the gallic acid present.

Five pounds and a half of the seeds being worked upon, by various digestions in water, alcohol, &c, and subsequent evaporations gave above eight ounces and a half of crystallized gallic acid.

When the pulp of the seeds had been exhausted by water, it was acted upon by alcohol, and a substance obtained by evaporation from the alcoholic solutions, which crystallized, and had the following properties: it was perfectly white; was insipid and inodorous; it fused at 70° C. (158° Fahrenheit); on cooling, it crystallized in mingling long-acicular forms; it dissolved in strong boiling alcohol, in all proportions, the solution on cooling yielding mammellated groups of acicular crystals; it is insoluble in water; it reddens moistened litmus paper; its solution in weak alcohol reddens infusion of litmus; it is quite soluble in oils and fatty bodies; it unites to salifiable bases, forming well characterized salts (soaps); when made into a taper, it burns like wax, with a fine white flame. This substance has all the physical and chemical characters of stearic acid, which therefore exists, ready formed, in the vegetable kingdom. Its quantity was rather more than two ounces.

When the pulp, thus far exhausted, was treated with ether, a fatty matter was obtained from it, fusing at 30° C. (86° Fahrenheit); soluble in hot ether to any extent; insoluble in rectified alcohol; liquefying in the mouth like cocoa butter; when formed into a candle,

* Journ. de Pharm., 1831, p. 437.

burning like tallow; having the consistence of tallow, and being the same nature as the butter of cocoa. The powdered grain trea with water yields a small portion of this butter in a very pure fresh state. The quantity obtained from the original quantity seed was one ounce and a half.

After all these operations the starch was separated by washing water; its quantity amounted to 32½ oz., or rather more than the weight of the dried seeds. When the recent seeds were wor with for starch, 1 lb. always yielded about 6 oz. of starch, and drying lost about 6 oz. of water.

Besides these substances the following were also obtained lignine above 5 oz.; gum 2½ oz.; tannin 200 grs., nearly; br resin 200 grs.; green resin 144 grs., and a little vegetable albun

M. Arequin then describes processes for obtaining gallic from the mango seed, either with or without the use of alcohol, for the preparation of ink with this substance instead of galls. obtained in abundance, the seeds may be very useful for these analogous purposes *.

17. On Lactic Acid.

Berzelius has exerted his talents in the explication and clearan the doubts which have existed relative to the existence of lactic independent of the acetic acid. Gmelin, on distilling fluids con ing lactic acid, obtained a product which feebly reddened l paper, and which, saturated by baryta and the solution evaporated, a white pellicle, which, when touched with sulphuric acid, evolv odour of acetic acid.

Berzelius repeated the experiment, and obtained the same but the acid odour never occurred unless muriatic acid was pre the results of the distillation. When pure lactic acid with wat distilled and the product evaporated, it reddened litmus paper, b a little of the acid had been carried over with the vapour tartaric acid was found to pass over in the same manner. In is extremely difficult to prevent a little of the contents of the in the finely divided state assumed, from passing over: but wl product of lactic acid was distilled a second time, not a trace passed over. If acetic acid had been present it would have d

The question then arose whether lactic acid was a comp acetic acid and an animal matter analogous to the sulpho- All attempts to prove this in the affirmative failed. Lactic ac heated up to the point at which extractive matter would brown, and then a current of ammonia passed over it, but no of ammonia passed forward in vapour, though that must have c if acetic acid had been present.

The following are the methods adopted by Berzelius to obt lactic acid. The acid alcoholic extract obtained from the H

* Journ. de Pharm., 1831, p. 421.

milk or meat is to be dissolved in strong alcohol and mixed with a strong alcoholic solution of tartaric acid, whilst any precipitate falls: being left for twenty-four hours in a cold place, the double tartrate separates. The solution being evaporated, the extract is to be dissolved in water, and well pulverised carbonate of lead added as long as it dissolves, and till the solution has a sweet taste; it is then to be acted upon by animal charcoal, and afterwards by sulphuretted hydrogen, to remove the lead. The liquid is to be evaporated that all sulphuretted hydrogen may be expelled; and then, mixed with hydrated protoxide of tin recently prepared, well washed and still moist, it is to be left several days in contact, with agitation at intervals. The sublactate of tin produced, when well washed, is to be decomposed by sulphuretted hydrogen, and thus the purest possible lactic acid has been obtained. In this way much acid remains in the solution and is virtually lost. Whether this is another acid, resulting from the partial decomposition of that under purification, or whether it forms a soluble perlactate of tin, is uncertain: but the liquid from over the protoxide of tin, when acted on by sulphuretted hydrogen, gives bi-sulphuret of tin.

Another process is to saturate the acid alcoholic extract with carbonate of potassa or soda: evaporate and heat the mass obtained in a sand bath till it fuses, becomes brown and evolves the odour of urine, and ultimately of herring, or roast meat: to dissolve in water, act by animal charcoal till colourless; filter, evaporate to dryness; dissolve in alcohol; decompose by tartaric acid; remove the excess of tartaric acid by carbonate of lead; precipitate the lead by sulphuretted hydrogen, and evaporate. In this way colourless acid is obtained, but it contains extractive matter, and is less pure than the former.

Lactic acid is colourless, inodorous; it possesses a sharp taste, rapidly diminishing on the addition of water till the taste is nearly lost. When evaporated at 212° F., until it loses nothing more, it becomes thick so as to flow with difficulty or even to be a soft solid. It attracts moisture from the atmosphere. When strongly heated, it becomes brown, boils slightly, evolves a suffocating odour like that of heated oxalic acid, blackens, swells, and leaves a bulky charcoal. It dissolves readily in alcohol and in small quantities in ether.

Many of the salts are uncrystallizable, or gummy; those of potassa, ammonia, magnesia, and zinc crystallized. They dissolve in alcohol, though sometimes with difficulty, especially if there be excess of base present*.

18. On Camphor and Camphoric Acid.—*M. J. Liebeg.*

The accounts given of the camphorates by R. Brandes and Bouillon-Lagrange are extremely different. These differences are, by M. Liebeg, referred to these chemists having used two different kinds of camphoric acid. Camphor, acted upon by concentrated nitric acid,

* Ann. de Chimie, xlvi. 420.

fuses into a yellow liquid; by long digestion this disap] the acid liquor, on cooling, deposits a large quantity of opa crystals, which when boiled with water communicate an camphor to it. This is the camphoric acid of Bouillon, an salts either slightly soluble or insoluble.

These crystals are a chemical combination of camphor phoric acid, and may be prepared at once by dissolving in camphoric acid fused at a gentle heat. If they be a second time by strong nitric acid, crystals more transp obtained, which give exactly the soluble salts described by Even this acid was found by M. Liebeg not to be quite therefore it was again heated with nitric acid until the obtained, when boiled with water, gave no odour of campl vapour.

Camphorate of lead was prepared with this acid and de by sulphuric acid; from 1105 parts of the camphor obtained 760 of the sulphate, the equivalent number of tl therefore 135.67. On analysing the camphorate of lead of copper for the composition of camphoric acid, it can follows:—

				Calculation	Expe
10 atoms of carbon		76.4370	or	56.29	56
15 ,,	hydrogen	9.3597	,,	6.89	6
5 ,,	oxygen	50.0000	,,	36.82	36
		135.7967		100.00	100

Many chemists, 'amongst which I counted myself,' says M calculate that 2 volumes of hydrogen are equal to an ato others consider 1 volume as an atom. Camphoric acid leave no doubt on this subject: it contains 15 atoms of single and only $7\frac{1}{2}$ of double volumes. Now $7\frac{1}{2}$ atoms could o from an error in analysis, but every possible care was take these: on the contrary, 15 atoms is in the simplest ratio other bodies present.

When camphor is acted on by nitric acid, there is no effer and no carbonic acid gas is disengaged. It would seem camphor was merely oxidized. M. Liebeg, therefore, camphor to see if the mere addition of oxygen would give stitution of camphoric acid above determined. His best ex] (of which he still speaks cautiously) gave

Carbon	81.763	or	1 atom
Hydrogen	9.702	,,	18 atoms
Oxygen	8.535	,,	12 ,,

The hydrogen and oxygen are, therefore, in the same r camphoric acid, and if it be supposed that, during the action acid on camphor, 5 atoms of oxygen be given, the acid wi

Hence camphor appears to act like a simple body, and there is no other case of a similar kind amongst organic bodies, except with indigo *.

19. Action of Heat on Acetate of Lead.—(*Matteuci.*)

When acetate of lead is heated it fuses at 135½° F., and boils at 212°. During the ebullition it loses its three proportionals of water, and solidifies nearly at the same temperature. When the temperature is still further raised fusion re-occurs at 536° F., the liquid boils for some time, becomes brownish, and again solidifies into a dull white uncrystalline mass, which is a trisacetate of lead. During the operation, acetic acid, with a little pyroacetic spirit, comes over, but at a later period only pyroacetic spirit, with abundance of carbonic acid, is obtained.

The pyroacetic spirit being analysed was found to contain hydrogen 6.4039; carbon 59.86; and oxygen 33.7361 per cent.=to 3 : 5 : 1 volume of these substances. This composition may be represented by 1 proportion of acetic acid 1 of water, and a substance composed of 6 proportions of carbon and 2 of hydrogen. Pyroacetic spirit soon begins to decompose, and when exposed only a few minutes to the air, becomes acid and milky; acetic acid and an oleaginous body (in appearance) are produced. When heated in contact with potassa or lime, acetates are formed and the oleaginous substance evolved. With chlorine it produces muriatic and acetic acids, and the same oleaginous body. The oleaginous principle has an aromatic odour, and by exposure to air becomes green; it is insoluble in water, but soluble in alcohol. It contains no chlorine, but is a binary compound of carbon and hydrogen †.

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§ III. *NATURAL HISTORY.*

1. Industry of Birds.

Dr. Steel, who resides near the mineral springs of Saratoga, has observed, that the river swallow (*Hirundo riparia*) can, when necessary, vary the construction of its nest. When it finds steep sandy banks, it excavates them, forming holes for its nest to which its enemies cannot obtain access. But when such a bank is wanting, it approaches the habitations of men, and though less familiar than the window swallow, it attaches its nest to granaries, cart houses, and similar places. Then it is obliged to build instead of excavate, and it does in fact gather materials and put them together. It appears too that this species has not essentially the habitudes indicated by its name: it can live and thrive wherever it can find subsistence, security, and society; (for isolated familes or solitary nests do not occur.) A

* Ann. de Chimie, xlvii. p. 95. † Ibid., xlvi. p. 429.

small colony, which in 1828 fixed itself at Saratoga, in rapidly, that in 1830 there were several hundred nests.

These animals, therefore, have innate faculties, to which of instinct may be given, and acquired faculties, variable to circumstances: they may create feeble arts (according t expression), and lose them as rapidly as they were ac circumstances be not favourable to the continuance of their

2. On the rapid Flight of Insects.

In passing along the Manchester and Liverpool railway a of about twenty-four miles an hour, ascertained by a stop observed one of the smaller humble-bees, I think the *Apis rupta*, flying for a considerable distance, and keeping pace train, apparently without the slightest effort; in fact, the little was going at a rate far more rapid than ours, for its ac ment was not in a straight line, but in that well known zigz of flight, observable when these insects are hovering from flower in search of food. Several house blue-bottle and h were also repeated visiters: our rapid motion seemed to manner of effect upon them, for, when it suited their purp darted onwards for a few feet or yards, or balanced th steadily over any given point, though in an instant, whene efforts relaxed, or they thought it expedient to part compa were far away in our rear. I should observe, moreover, wind at the time was blowing obliquely against us with a c such strength, that I occasionally had some difficulty in kee hat on. Under all circumstances, therefore, of the wind's op and their irregular motion, I considered that the locomotive of these insects could not be well less than from thirty to fo an hour. Compared with the beautifully arranged muscular of these minute beings in the creation, how insignificant a which science, with all its advantages, has hitherto been accomplish by mechanical means†!—D. T.

[It is to be presumed that these insects were not in sheltered by the train, *i.e.* were not to leeward of or beh advancing part, for then they would find no difficulty in keep with the engines. But it would have been better if the fact h distinctly noticed and stated.—Ed.]

3. Useful Astringent in Cases of Mercurial Saliva (*M. Virey.*)

It often occurs that in cases of mercurial treatment the astringents of tea, myrrh, bark, borax, alum, and even nitric a insufficient to arrest the flow of saliva, in consequence o injurious results have occurred, very difficult to remedy.

* Rev. Ency., l. p. 173. † Phil. Mag. N.S., x. p. 15

been found in the United States, that the decoction of the bark of a sumach (*Rhus glabrum*) has, in this respect, remarkable powers. Care must be taken not to confound this species with the *Rhus vernix*, which very much resembles it, for the latter is acrid, and, according to Dr. Fahnestock, produces injurious effects. In the genus of sumach there are aromatic as well as poisonous species. Of the former kind are the *Rhus suaveolens*, and *Rhus aromaticum*, both of North America *.

4. Relief for the Tooth-ache.

The following account is by Dr. Ryan, who himself testifies to the efficacy of the remedy recommended. 'Like many of our best remedies, that which I proceed to notice (for the tooth-ache) was discovered by accident. A gentleman who attends my lectures (Mr. Myers, of Newington Causeway) had frequently applied sulphuric acid to his tooth with some relief, but on one occasion, he, in a moment of confusion, took down the next bottle to his remedy, which contained nitric acid: to his great surprise he experienced immediate relief and without the slightest pain. Since that period he has not suffered from tooth-ache, though three years have now elapsed. During the last winter he informed me of the success of this remedy, which induced me to try it while labouring under the most intense pain from tooth-ache. The effect was immediate, and no pain whatever was induced. I have since used it in numerous cases, and invariably with complete success. In some instances the disease does not return for days, or weeks, and in others not for months.'

The best mode of employing it is by means of lint wrapped round a probe and moistened with the acid, which is then to be slowly applied to the cavity of the tooth, care being taken not to touch the other teeth, the gums, or the cheeks. On withdrawing the probe and inquiring how the patient feels, the usual reply is, 'The pain is entirely gone.' The mouth is next to be washed with tepid water. The acid should be gradually applied to the whole cavity of the tooth, or otherwise a second application will be required before complete relief will be obtained.

This remedy may be used when the gum and cheek are inflamed so as to preclude the possibility of extraction. In cases where the diseased fang remains, and when the caries faces the adjacent tooth, it obviates the necessity of extraction in all cases of hollow teeth; which all practitioners declare to be desirable if possible, and it enables the dentist to perform the operation of 'stopping or filling teeth' much sooner than he can otherwise accomplish. In a word, it will alleviate a vast deal of human suffering and supersede a most painful operation. It does not accelerate the decay of the tooth to which it is applied†.

* Jour. de Pharm., 1831, p. 391. † Lon. Med. Jour., vii. p. 56.

5. Poisoning by the (sebacic) Acid of Goose-Gre.

On the 2d of April, 1829, Dr. Siedler was called to MM. H——, and their children. On his arrival he found t brothers H——, one aged thirty-one, the second twenty-eigh and the two children of the first, one a girl æt. four, the othe æt. two and a half, all presenting the following symptoms sweat, anxiety, vertigo, general paleness and prostration of st eyes sunken, and pupils dilated; burning pain was felt in th part of the belly, increased by pressure; violent vomiting suc by ardent thirst, for which the patients had drunk large quant milk, which was thrown up without producing any effect; dry, involuntary discharge of urine and fæces.

The eldest brother was insensible for six minutes; his resp was scarcely visible, his pulse imperceptible, and the heart's exceedingly weak. The second brother had vomited blood times, but he experienced less abdominal pain than the oth the little boy the globes of the eyes were turned upwards, t livid, and the pulse scarcely sensible. Lastly, the symptoms little girl were the mildest of all. M. Siedler suspected at on these accidents were occasioned by the use of a certain quan goose-grease, which had been employed in the preparation of meat, of which the four patients had eaten shortly before the toms began. An emulsion, containing hyoscyamus, was pres and on the 9th of April all had recovered.

The vomited matters were subjected to chemical analysis were strongly acid, but contained no metallic poison: but the f ing facts induced Dr. Siedler to attribute the illness to the ef sebacic acid. The lady of the house had made use of goose- to dress some veal, and all the persons who partook of the di quickly sick. The lady herself, who had barely tasted it, fel disagreeable, that she took no more. None of the grease was suspected to have caused the accident remained for exami the pot which contained it having been entirely emptied and c out; but on examining the same kind of grease contained in other pots, it was found to exhale a strong repulsive odour, reddened strongly blue paper tinged by turnsole. Three oun this grease were given to a vigorous, well-formed dog: an hour his extremities became violently convulsed; he cried piteous refused to eat, his eyes were suffused, pupils dilated, skin col arterial pulsations scarcely perceptible. In this state he cont for thirty hours, after which he slowly recovered *.

6. Influence of atmospheric Electricity on the Ey

We see peculiar appearances in weak and morbidly sensitive before the breaking out of a violent storm, showing the po influence of an atmosphere which has now attained its maxim

* Hufeland's Journal.

electricity, and I am acquainted with several persons who are able, from a certain premonitory feeling of their weak eyes, to predict a thunder-storm, infallibly, a considerable time beforehand. As every experienced oculist is convinced of the bad effects of an atmosphere of this kind, he will never extract the cataract during the approach of a thunder-storm *.

7. Native Country of the Potatoe.

The following observations are made upon this highly interesting subject by Mr. Cruickshanks, in Dr. Hooker's *Botanical Miscellany.*

Mr. Lambert, in the tenth volume of Brande's Journal, and in the appendix to his splendid work on the genus *Pinus*, has collected many valuable facts which prove that the potatoe is found wild in several parts of America, and among others in Chili and Peru. Don José Pavon, in a letter to Mr. Lambert, says, 'The *Solanum tuberosum* grows wild in the environs of Lima, and fourteen leagues from Lima on the coast; and I myself have found it in the kingdom of Chili,'—and Mr. Lambert adds, 'I have lately received from Mr. Pavon very fine wild specimens of *Solanum tuberosum*, collected by himself in Peru.' There is also a note from Mr. Lambert on the same subject, in the third volume of the *New Edin. Phil. Journ.*, with an extract from a letter of Mr. Caldcleugh, who sent tubers of the wild plant, some years ago, from Chili to the Horticultural Society.

But it is frequently objected, that in some of those countries where the *potatoe* is found wild, it may, like many other species met with in that state in America, be an *introduced*, not an *indigenous* plant. There are, however, many reasons for believing that it is really indigenous in Chili, and that wild specimens found there have not been accidentally propagated from any cultivated variety. In that country it is generally found in steep, rocky places, where it could never have been cultivated, and where its accidental introduction is almost impossible. It is very common about Valparaiso, and I have noticed it along the coast for fifteen leagues to the northward of that port; how much farther it may extend north or south, I know not. It chiefly inhabits the cliffs and hills near the sea, and I do not recollect to have seen it at more than two or three leagues from the coast. But there is one peculiarity in the wild plant that I have never seen noticed in print, that its flowers are always *pure white*, free from the purple tint so common in the cultivated varieties, and this, I think, is a strong evidence of its native origin. Another proof may be drawn from the fact, that while it is often met with in mountainous places, remote from cultivated ground, it is not seen in the immediate neighbourhood of the fields and gardens where it is planted, *unless a stream of water run through the ground, which may carry tubers to uncultivated spots.*

Having observed the distribution of this and other plants through the agency of the streams employed for irrigating the land, I am led to think, that the wild specimens found near Lima may have had

* Med. Journ., 1831, p. 76.

similar origin. If they occurred in the valley, this is more tha probable, as almost the whole of the land is either cultivated by irr gation, or the uncultivated spots are overflowed when the river swelled by the rains in the interior. I remember a curious instan of this sort of vegetable colonization. In the vineyards of Chili, is customary, in order to economise the land, to sow *lucerne* amo the vines, to the great injury of the latter, as it prevents the grou from being ploughed or hoed. An intelligent landowner, who h travelled in France, and observed the beneficial effects of turning and manuring the land, determined to adopt the same system i large vineyard he was planting near Santiago, and gave orders to *mayor domo* not to sow *lucerne* seed in it as usual. On visiting estate some months afterwards, he was astonished to find the la covered with young plants of the forbidden pasture, although n had been sown; and on investigating the matter, it was found the stream which irrigated his grounds passed first through sev *lucerne* fields in another part of the valley, from which it had car and disseminated *seed* over the whole vineyard.

Humboldt, who has bestowed such unwearied attention on subject of plants cultivated in the New World, (but whose work published previous to that of Mr. Lambert,) denies that the *pota* indigenous to Peru. In his *Essai Politique sur le Royaume Nouvelle Espagne*, he says, 'J'observe d'abord, pour ne cons ici que des faits exacts, que la *pomme de terre* n'est pas indigè Pérou, et qu'elle ne se trouve pas nulle part sauvage dans la par la Cordillère qui est située sous les tropiques. Nous avon Bonpland et moi, herborisé sur le dos et sur la pente des depuis les 5° nord, jusqu'aux 12° sud; nous avons pris des in tions chez des personnes qui ont examiné cette chaîne de mon colossales jusqu'à La Paz et à Oruro, et nous sommes sû dans cette vaste étendue de terrein il ne végète spontanément espèce de Solanée à racines nourrissantes.'—'MM. Ruiz et dont l'autorité est d'un grand poids, disent avoir trouvé la po terre dans les terrains cultivés, *in cultis*, et non dans les forêt le dos des montagnes,' page 400. The last paragraph, how at variance with the letter of Don José to Mr. Lambert, ar appears to be inferred from what Ruiz and Pavon say on the in the *Flora Peruviana*, than those authors intended. The in that work, after the description of the *Solanum tuberos* follows:—'Habitat in Peruviæ et Chilensis regni cultis, *et in Chancay, ad Jequan et Pasamayo prædia.*' If they had on it in cultivated land, the first part of this passage would h sufficient; but the context leaves it to be understood that tha stance does not apply to its locality at Chancay.

Chancay is a town on the coast of Peru, which gives its the surrounding district or jurisdiction, in which the estates and Pasamayo are situated, and it is doubtless the place allu Don José's letter, being about the distance he mentions Lima. There is a great extent of cultivated land in the n

hood, irrigated from the river of Pasamayo, (called also the river of Chancay,) but Ruiz and Pavon say, they found the plant *in the hills*, where, as I have before observed, there is no cultivation. As nothing, however, is stated of the nature of the hills, nor of the height at which the plant occurs above the valley, there is still room to suspect that it may have been accidentally introduced, and, indeed, the Indians formerly brought water upon the land from a considerable distance, at a much greater elevation than any that is irrigated at the present day.

Upon the whole, it may be safely concluded that this important vegetable is really indigenous to Chili; but with respect to Peru, some further evidence appears necessary to remove all doubt on the subject. The question can only be decided by ascertaining the exact situations in which the plants present themselves at Lima and Chancay, especially with respect to land that is or has been cultivated. It would be interesting, too, to know the *colour* of the flowers.

8. New Forms of Cellular Tissue.

Dr. J. E. Purkinje has ascertained that the tissue which constitutes the lining of the case of an anther is of a peculiar kind. It consists generally of vesicular cellular tissue, the membranous walls of which are marked with spiral fibres, coiled up in the inside from the one end to the other; sometimes the fibres are placed like a number of ribs passing from the base to the apex, without any trace of a spiral direction. Occasionally the membrane of the tissue disappears and fibres only remain; and in some cases the whole of the tissue is reduced to a number of points sticking up from the inside of the anther. It is probable that these and similar observations will throw much light upon the analogy that exists between cellular and vascular tissue.

9. Functions of Spiral Vessels.

From the researches of Dr. L. W. T. Bischoff of Bonn it appears that the spiral vessels of plants contain no fluid, but serve exclusively to convey air, and that this air has from 7 to 8 per cent. more oxygen than the atmosphere. He found by very delicate and repeated experiments, that the air from the spiral vessels of *malva arborea* contained 27.9 per cent. of oxygen, and *cucurbita pepo*, at two different times, 29.8 and 27.9 per cent. From the researches of the same anthor, it seems that the spiral vessel is a delicate pellucid membranous tube, within which a spiral fibre is generated; that when the whole surface of the tube is filled up by the coils of the spiral fibre, a true spiral vessel is the result, and that spurious spiral vessels, or ducts, with all their modifications, are formed by dislocations or separations of the spires within the membrane. Dotted ducts, for instance, are, according to Dr. Bischoff, caused by the separation of the spiral fibre into minute points.

10. Cryptogamia in Molasses.—*Virey.*

Van Dyk and Van Beck of Utrecht have observed a black
in molasses which rapidly extends and enlarges, and is, ac
them, a cryptogamous plant identical with the *conferva*
of Agardh (*Syst. Algarum,* Lund., 1824); or more re
syncollesia mucoroïdes (in *algæ confervoideæ funginæ.*)
syncollesia of Nees is thus characterised by Agardh: *glob
tissimi, in fila repentia, cæspitosa, coadunati, leviter*
The Dutch authors make a species of that mentioned abo
lesia sacchari; different from the *hyphomycetus* of M
aleuryoma granulosum of Martius, and the *sporotrichu*
and *S. vitellinum* of Link, all of which are observed in mc

These mouldy productions appear to be due to the imp
with which the sugar moulds are washed: lime water kills

11. Algerine Sirocco.

When the south wind blows at Algiers, the temperature ri
5° or even 10° C. (9° or 18° F.) On the 17th of Septem
the thermometer rose to 39° C. (104° F.) in the shade
then as if all were in a furnace, and men and animals fou
tion difficult. Captain Boissel, who superintended the wor
day in the suburb of Babazon, remarked that drunken men
less; those who were not so drunk resisted the effect a li
last fell; and those who had drunk only a little too muc
from very violent head-ache and were obliged to sit down

12. Atmospheric Electricity.

Storms are not very common in the climate of Algiers, n
several occurred (towards the end of 1830), breaking up
Atlas. On the 8th of October, in the evening, the air toward
was powerfully charged with electricity, the whole horiz
on fire, and the thunder was continual. At the same mome
white light was seen at the extremities of all the poles of th
which were within Algiers, or in the neighbouring
which continued for half an hour. Artillery officers who
menading bare-headed on the terrace of fort Babazon
much astonished to feel their hair rise upright on their
to observe, with those which were visible, a small star of l
extremity of each. When they raised their hands in the
formed at the extremities of their fingers; these disappe
the hands were lowered. During the whole time of t
every one felt nervous sensations, and great lassitude over
body, but especially in the legs. These effects were obs
are described by M. Royet ‡.

* Journ. de Pharm., 1831, p. 393. † Revue Ency., l., p. 619.